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ADDRESS

BY

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New Archæological lights on the Origins of Civilisation in Europe: its Magdalenian forerunners in the South-West and Ægean Cradle.

Et quasi cursores vitaï lampada tradunt.

When I was asked on behalf of the Council of the British Association to occupy the responsible post of President at the Meeting in this great city—the third that has taken place here—I was certainly taken by surprise; the more so as my own subject of research seemed somewhat removed from what may be described as the central interests of your body. The turn of Archæology, however, I was told, had come round again on the rota of the sciences represented; nor could I be indifferent to the fact that the last Presidential Address on this theme had been delivered by my father at the Toronto Meeting of 1897.

Still, it was not till after considerable hesitation that I accepted the honour. Engaged as I have been through a series of years in the work of excavation in Crete—a work which involved not only the quarrying but the building up of wholly new materials and has entailed the endeavour to classify the successive phases of a long, continuous story—absorbed and fascinated by my own investigations—I am oppressed with the consciousness of having been less able to keep pace with the progress of fellow explorers in other departments or to do sufficient justice to their results. I will not dwell, indeed, on those disabilities that result to myself from present calls and the grave preoccupations of the hour, that to a greater or less extent must affect us all.

But Archæology—the research of ancient civilisations—when the very foundations of our own are threatened by the New Barbarism! The investigation of the ruins of the Past—at a time when Hell seems to have been let loose to strew our Continent with havoc beyond the dreams of Attila! 'The Science of the Spade'—at a moment when

that Science confronts us at every hour with another and a sterner significance! The very suggestion of such a subject of discourse might seem replete with cruel irony.

And yet, especially as regards the prehistoric side of Archæology, something may be said for a theme which, in the midst of Armageddon, draws our minds from present anxieties to that still, passionless domain of the Past which lies behind the limits even of historic controversies. The Science of Antiquity as there seen in its purest form depends, indeed, on evidence and rests on principles [illegible] from those of the sister Science of Geology. Its methods are stratigraphic. As in that case the successive deposits and their characteristic contents—often of the most fragmentary kind—enable the geologist to reconstruct the fauna and flora, the climate and physical conditions, of the past ages of the world, and to follow out their gradual transitions or dislocations, so it is with the archæologist in dealing with unwritten history.

In recent years—not to speak of the revelations of Late Quaternary culture on which I shall presently have occasion to dwell—in Egypt, in Babylonia, in Ancient Persia, in the Central Asian deserts, or, coming nearer home, in the Ægean lands, the patient exploration of early sites, in many cases of huge stratified mounds, the unearthing of buried buildings, the opening of tombs, and the research of minor relics, has reconstituted the successive stages of whole fabrics of former civilisation, the very existence of which was formerly unsuspected. Even in later periods, Archæology, as a dispassionate witness, has been continually checking, supplementing, and illustrating written history. It has called back to our upper air, as with a magician's wand, shapes and conditions that seemed to have been irrevocably lost in the night of Time.

Thus evoked, moreover, the Past is often seen to hold a mirror to the Future—correcting wrong impressions—the result of some temporary revolution in the whirligig of Time—by the more permanent standard of abiding conditions, and affording in the solid evidence of past well-being the 'substance of things hoped for.' Nowhere, indeed, has this been more in evidence than in that vexed region between the Danube and the Adriatic, to-day the home of the Serbian race, to the antiquarian exploration of which many of the earlier years of my own life were devoted.

What visions, indeed, do those investigations not recall! Imperial cities, once the seats of wide administration and of prolific mints, sunk to neglected villages, vestiges of great engineering works, bridges, aqueducts, or here a main line of ancient highway hardly traceable even as a track across the wilderness! Or, again, the signs of medieval revival above the Roman ruins—remains of once populous mining

centres scattered along the lone hillside, the shells of stately churches with the effigies, bullet-starred now, of royal founders, once champions of Christendom against the Paynim—nay, the actual relics of great rulers, lawgivers, national heroes, still secreted in half-ruined monastic retreats!

Sunt lacrimæ rerum et mentem mortalia tangunt:

Even the archæologist incurs more human debts, and the evocation of the Past carries with it living responsibilities!

It will be found, moreover, that such investigations have at times a very practical bearing on future developments. In connexion with the traces of Roman occupation I have recently, indeed, had occasion to point out [1] that the section of the great Roman road that connected the Valleys of the Po and Save across the lowest pass of the Julians, and formed part of the main avenue of communication between the Western and the Eastern provinces of the Empire, has only to be restored in railway shape to link together a system of not less value to ourselves and our Allies. For we should thus secure, via the Simplon and Northern Italy, a new and shorter Overland Route to the East, in friendly occupation throughout, which is to-day diverted by unnatural conditions past Vienna and Budapest. At a time when Europe is parcelled out by less cosmopolitan interests the evidence of Antiquity here restores the true geographical perspective.

Whole provinces of ancient history would lie beyond our ken—often through the mere loss of the works of classical authors—were it not for the results of archæological research. At other times again it has redressed the balance where certain aspects of the Ancient World have been brought into unequal prominence, it may be, by mere accidents of literary style. Even if we take the Greek World, generally so rich in its literary sources, how comparatively little should we know of its brilliant civilisation as illustrated by the great civic foundations of Magna Graecia and Sicily if we had to depend on its written sources alone. But the noble monuments of those regions, the results of excavation, the magnificent coinage—a sum of evidence illustrative in turn of public and private life, of Art and Religion, of politics and of economic conditions—have gone far to supply the lacuna.

Look, too, at the history of the Roman Empire—how defective and misleading in many departments are the literary records! It has been by methodical researches into evidence such as the above—notably in the epigraphic field—that the most trustworthy results have been worked out.

Take the case of Roman Britain. Had the lost books of Ammianus

[1] 'The Adriatic Slavs and the Overland Route to Constantinople.' *Geographical Journal*, 1916, p. 241 *seqq*.

relating to Britain been preserved we might have had, in his rugged style, some partial sketch of the Province as it existed in the age of its most complete Romanisation. As it is, so far as historians are concerned, we are left in almost complete darkness. Here, again, it is through archæological research that light has penetrated, and thanks to the thoroughness and persistence of our own investigators, town sites such as Silchester in Roman Britain have been more completely uncovered than those of any other Province.[2] Nor has any part of Britain supplied more important contributions in this field than the region of the Roman Wall, that great limitary work between the Solway and the mouth of the Tyne that once marked the Northernmost European barrier of civilised dominion.

Speaking here, on the site of Hadrian's bridge-head station that formed its Eastern key, it would be impossible for me not to pay a passing tribute, however inadequate, to the continuous work of exploration and research carried out by the Society of Antiquaries of Newcastle, now for over a hundred years in existence, worthily seconded by its sister Society on the Cumbrian side, and of which the volumes of the respective *Proceedings* and *Transactions*, *Archæologia Æliana*, and last but not least the *Lapidarium Septentrionale*, are abiding records. The basis of methodical study was here the Survey of the Wall carried out, together with that of its main military approach, the Watling Street, by MacLauchlan, under the auspices of Algernon, fourth Duke of Northumberland. And who, however lightly touching on such a theme, can overlook the services of the late Dr. Collingwood Bruce, the Grand Old Man, not only of the Wall itself, but of all pertaining to Border Antiquities, distinguished as an investigator for his scholarship and learning, whose lifelong devotion to his subject and contagious enthusiasm made the Roman Wall, as it had never been before, a household word?

New points of view have arisen, a stricter method and a greater subdivision of labour have become imperative in this as in other departments of research. We must, therefore, rejoice that local explorers have more and more availed themselves of the co-operation, and welcomed the guidance of those equipped with comparative knowledge drawn from other spheres. The British Vallum, it is now realised, must be looked at with perpetual reference to other frontier lines, such as the Germanic or the Rhætian *limes*; local remains of every kind have to be correlated with similar discoveries throughout the length and breadth of the Roman Empire.

This attitude in the investigation of the remains of Roman Britain—the promotion of which owes so much to the energy and experience of Professor Haverfield—has in recent years conducted excavation to

[2] See Haverfield: *Roman Britain in* 1913, p. 86.

specially valuable results. The work at Corbridge, the ancient Corstopitum, begun in 1906, and continued down to the autumn of 1914, has already uncovered [illegible] a great part of its area the largest urban centre—civil as well as military in character—on the line of the Wall, and the principal store-base of its stations. Here, together with well-built granaries, workshops, and barracks, and such records of civic life as are supplied by sculptured stones and inscriptions, and the double discovery of hoards of gold coins, has come to light a spacious and massively constructed stone building, apparently a military store-house, worthy to rank beside the bridge-piers of the North Tyne, among the most imposing monuments of Roman Britain. There is much here, indeed, to carry our thoughts far beyond our insular limits. On this, as on so many other sites along the Wall, the inscriptions and reliefs take us very far afield. We mark the grave-stone of a man of Palmyra, an altar of the Tyrian Hercules—its Phœnician Baal—a dedication to a pantheistic goddess of Syrian religion and the rayed effigy of the Persian Mithra. So, too, in the neighbourhood of New-castle itself, as elsewhere on the Wall, there was found an altar of Jupiter Dolichenus, the old Anatolian God of the Double Axe, the male form of the divinity once worshipped in the prehistoric Labyrinth of Crete. Nowhere are we more struck than in this remote extremity of the Empire with the heterogeneous religious elements, often drawn from its far Eastern borders, that before the days of the final advent of Christianity, Roman dominion had been instrumental in diffusing. The Orontes may be said to have flowed into the Tyne as well as the Tiber.

I have no pretension to follow up the various affluents merged in the later course of Greco-Roman civilisation, as illustrated by these and similar discoveries throughout the Roman World. My own recent researches have been particularly concerned with the much more ancient cultural stage—that of prehistoric Crete—which leads up to the Greco-Roman, and which might seem to present the problem of origins at any rate in a less complex shape. The marvellous Minoan civilisation that has there come to light shows that Crete of four thousand years ago must unquestionably be regarded as the birth-place of our European civilisation in its higher form.

But are we, even then, appreciably nearer to the fountain-head?

A new and far more remote vista has opened out in recent years, and it is not too much to say that a wholly new standpoint has been gained from which to survey the early history of the human race. The investigations of a brilliant band of prehistoric archæologists, with the aid of representatives of the sister sciences of Geology and Palæontology, have brought together such a mass of striking materials as to place the evolution of human art and appliances in the last Quaternary Period on a far higher level than had even been suspected previously.

Following in the footsteps of Lartet and after him Rivière and Piette, Professors Cartailhac, Capitan, and Boule, the Abbé Breuil, Dr. Obermeier and their fellow investigators have revolutionised our knowledge of a phase of human culture which goes so far back beyond the limits of any continuous story, that it may well be said to belong to an older World.

To the engraved and sculptured works of Man in the 'Reindeer Period' we have now to add not only such new specialities as are exemplified by the moulded clay figures of life-size bisons in the Tuc d'Audoubert Cave, or the similar high reliefs of a procession of six horses cut on the overhanging limestone brow of Cap Blanc, but whole galleries of painted designs on the walls of caverns and rock shelters.

So astonishing was this last discovery, made first by the Spanish investigator Señor de Sautuola—or rather his little daughter—as long ago as 1878, that it was not till after it had been corroborated by repeated finds on the French side of the Pyrenees—not, indeed, till the beginning of the present century—that the Palæolithic Age of these rock paintings was generally recognised. In their most developed stage, as illustrated by the bulk of the figures in the Cave of Altamira itself, and in those of Marsoulas in the Haute Garonne, and of Font de Gaume in the Dordogne, these primeval frescoes display not only a consummate mastery of natural design but an extraordinary technical resource. Apart from the charcoal used in certain outlines, the chief colouring matter was red and yellow ochre, mortars and palettes for the preparation of which have come to light. In single animals the tints are varied from black to dark and ruddy brown or brilliant orange, and so, by fine gradations, to paler nuances, obtained by scraping and washing. Outlines and details are brought out by white incised lines, and the artists availed themselves with great skill of the reliefs afforded by convexities of the rock surface. But the greatest marvel of all is that such polychrome masterpieces as the bisons, standing and couchant, or with limbs huddled together, of the Altamira Cave, were executed on the ceilings of inner vaults and galleries where the light of day has never penetrated. Nowhere is there any trace of smoke, and it is clear that great progress in the art of artificial illumination had already been made. We now know that stone lamps, decorated in one case with the engraved head of an ibex, were already in existence.

Such was the level of artistic attainment in South-Western Europe, at a modest estimate some ten thousand years earlier than the most ancient monuments of Egypt or Chaldæa! Nor is this an isolated phenomenon. One by one, characteristics, both spiritual and material, that had been formerly thought to be the special marks of later ages of mankind have been shown to go back to that earlier World. I

myself can never forget the impression produced on me as a privileged spectator of a freshly uncovered interment in one of the Balzi Rossi Caves—an impression subsequently confirmed by other experiences of similar discoveries in these caves, which together first supplied the concordant testimony of an elaborate cult of the dead on the part of Aurignacian Man. Tall skeletons of the highly-developed Cro-Magnon type lay beside or above their hearths, and protected by great stones from roving beasts. Flint knives and bone javelins had been placed within reach of their hands, chaplets and necklaces of sea-shells, fish-vertebræ, and studs of carved bone had decked their persons. With these had been set lumps of iron peroxide, the red stains of which appeared on skulls and bones, so that they might make a fitting show in the Under-world.

'Colours, too, to paint his body,
Place within his hand,
That he glisten, bright and ruddy,
In the Spirit-Land!' [3]

Nor is it only in this cult of the departed that we trace the dawn of religious practices in that older World. At Cogul we may now survey the ritual dance of nine skirted women round a male Satyr-like figure of short stature, while at Alpera a gowned sister ministrant holds up what has all the appearance of being a small idol. It can hardly be doubted that the small female images of ivory, steatite, and crystalline talc from the same Aurignacian stratum as that of the Balzi Rossi interments, in which great prominence is given to the organs of maternity, had some fetichistic intention. So, too, many of the figures of animals engraved and painted on the inmost vaults of the caves may well have been due, as M. Salomon Reinach has suggested, to the magical ideas prompted by the desire to obtain a hold on the quarries of the chase that supplied the means of livelihood.

In a similar religious connexion may be taken the growth of a whole family of signs, in some cases obviously derivatives of fuller pictorial originals, but not infrequently simplified to such a degree that they resemble or actually reproduce letters of the alphabet. Often they occur in groups like regular inscriptions, and it is not surprising that in some quarters they should have been regarded as evidence that the art of writing had already been evolved by the men of the Reindeer Age. A symbolic value certainly is to be attributed to these signs, and it must at least be admitted that by the close of the late Quaternary Age considerable advance had been made in hieroglyphic expression.

The evidences of more or less continuous civilised development reaching its apogee about the close of the Magdalenian Period have been

[3] Schiller, *Nadowessier's Todtenlied.*

constantly emerging from recent discoveries. The recurring 'tectiform' sign had already clearly pointed to the existence of huts or wigwams; the 'scutiform' and other types record appliances yet to be elucidated, and another sign well illustrated on a bone pendant from the Cave of St. Marcel has an unmistakable resemblance to a sledge.[4] But the most astonishing revelation of the cultural level already reached by primeval man has been supplied by the more recently discovered rock paintings of Spain. The area of discovery has now been extended there from the Province of Santander, where Altamira itself is situated, to the Valley of the Ebro, the Central Sierras, and to the extreme South-Eastern region, including the Provinces of Albacete, Murcia, and Almeria, and even to within the borders of Granada.

One after another, features that had been reckoned as the exclusive property of Neolithic or later Ages are thus seen to have been shared by Palæolithic Man in the final stage of his evolution. For the first time, moreover, we find the productions of his art rich in human subjects. At Cogul the sacral dance is performed by women clad from the waist downwards in well-cut gowns, while in a rock-shelter of Alpera,[5] where we meet with the same skirted ladies, their dress is supplemented by flying sashes. On the rock painting of the Cueva de la Vieja, near the same place, women are seen with still longer gowns rising to their bosoms. We are already a long way from Eve!

It is this great Alpera fresco which, among all those discovered, has afforded most new elements. Here are depicted whole scenes of the chase in which bow-men—up to the time of these last discoveries unknown among Palæolithic representations—take a leading part, though they had not as yet the use of quivers. Some are dancing in the attitude of the Australian Corroborees. Several wear plumed head-dresses, and the attitudes at times are extraordinarily animated. What is specially remarkable is that some of the groups of these Spanish rock paintings show dogs or jackals accompanying the hunters, so that the process of domesticating animals had already begun. Hafted axes are depicted as well as cunningly-shaped throwing sticks. In one case at least we see two opposed bands of archers—marking at any rate a stage in social development in which organised warfare was possible—the beginnings, it is to be feared, of 'kultur' as well as of culture!

Nor can there be any question as to the age of these scenes and figures, by themselves so suggestive of a much later phase of human history. They are inseparable from other elements of the same group,

[4] This interpretation suggested by me after inspecting the object in 1902 has been approved by the Abbé Breuil (*Anthropologie*, XIII., p. 152) and by Prof. Sollas, *Ancient Hunters*², 1915, p. 480.

[5] That of Carasoles del Bosque; Breuil, *Anthropologie*, XXVI., 1915, p. 329 *seqq.*

the animal and symbolic representations of which are shared by the contemporary school of rock-painting north of the Pyrenees. Some are overlaid by palimpsests, themselves of Palæolithic character. Among the animals actually depicted, moreover, the elk and bison distinctly belong to the Late Quaternary fauna of both regions, and are unknown there to the Neolithic deposits.

In its broader aspects this field of human culture, to which, on the European side, the name of Reindeer Age may still on the whole be applied, is now seen to have been very widespread. In Europe itself it permeates a large area—defined by the boundaries of glaciation—from Poland, and even a large Russian tract, to Bohemia, the upper course of the Danube and of the Rhine, to South-Western Britain and South-Eastern Spain. Beyond the Mediterranean, moreover, it fits on under varying conditions to a parallel form of culture, the remains of which are by no means confined to the Cis-Saharan zone, where incised figures occur of animals like the long-horned buffalo (*Bubalus antiquus*) and others long extinct in that region. This Southern branch may eventually be found to have a large extension. The nearest parallels to the finer class of rock-carvings as seen in the Dordogne are, in fact, to be found among the more ancient specimens of similar work in South Africa, while the rock-paintings of Spain find their best analogies among the Bushmen.

Glancing at this Late Quaternary culture as a whole, in view of the materials supplied on the European side, it will not be superfluous for me to call attention to two important points which some observers have shown a tendency to pass over.

Its successive phases, the Aurignacian, the Solutrean, and the Magdalenian, with its decadent Azilian offshoot—the order of which may now be regarded as stratigraphically established—represent on the whole a continuous story.

I will not here discuss the question as to how far the disappearance of Neanderthal Man and the close of the Moustierian epoch represents a 'fault' or gap. But the view that there was any real break in the course of the cultural history of the Reindeer Age itself does not seem to have sufficient warrant.

It is true that new elements came in from more than one direction. On the old Aurignacian area, which had a trans-Mediterranean extension from Syria to Morocco, there intruded on the European side—apparently from the East—the Solutrean type of culture, with its perfected flint-working and exquisite laurel-leaf points. Magdalenian Man, on the other hand, great as the proficiency that he attained in the carving of horn and bone, was much behind in his flint-knapping. That there were dislocations and temporary set-backs is evident. But on every side we still note transitions and reminiscences. When,

moreover, we turn to the most striking features of this whole cultural phase, the primeval arts of sculpture, engraving, and painting, we see a gradual upgrowth and unbroken tradition. From mere outline figures and simple two-legged profiles of animals we are led on step by step to the full freedom of the Magdalenian artists. From isolated or disconnected subjects we watch the advance to large compositions, such as the hunting scenes of the Spanish rock-paintings. In the culminating phase of this art we even find impressionist works. A brilliant illustration of such is seen in the galloping herds of horses, lightly sketched by the engraver on the stone slab from the Chaumont Grotto, depicting the leader in each case in front of his troop, and its serried line—straight as that of a well-drilled battalion—in perspective rendering. The whole must be taken to be a faithful memory sketch of an exciting episode of prairie life.

The other characteristic feature of the culture of the Reindeer Age that seems to deserve special emphasis, and is almost the corollary of the foregoing, is that it cannot be regarded as the property of a single race. It is true that the finely built Cro-Magnon race seems to have predominated, and must be regarded as an element of continuity throughout, but the evidence of the co-existence of other human types is clear. Of the physical characteristics of these it is not my province to speak. Here it will be sufficient to point out that their interments, as well as their general associations, conclusively show that they shared, even in its details, the common culture of the Age, followed the same fashions, plied the same arts, and were imbued with the same beliefs as the Cro-Magnon folk. The negroid skeletons intercalated in the interesting succession of hearths and interments of the Grotte des Enfants at Grimaldi had been buried with the same rites, decked with the same shell ornaments, and were supplied with the same red colouring matter for use in the Spirit World, as we find in the other sepultures of these caves belonging to the Cro-Magnon race. Similar burial rites were associated in this country with the 'Red Lady of Paviland,' the contemporary Aurignacian date of which is now well established. A like identity of funeral custom recurred again in the sepulture of a man of the 'Brünn' race on the Eastern boundary of this field of culture.

In other words, the conditions prevailing were analogous to those of modern Europe. Cultural features of the same general character had imposed themselves on a heterogeneous population. That there was a considerable amount of circulation, indeed—if not of primitive commerce—among the peoples of the Reindeer Age is shown by the diffusion of shell or fossil ornaments derived from the Atlantic, the Mediterranean, or from inland geological strata. Art itself is less the property of one or another race than has sometimes been imagined—

indeed, if we compare those products of the modern carver's art that have most analogy with the horn and bone carvings of the Cave Men and rise at times to great excellence—as we see them, for instance, in Switzerland or Norway—they are often the work of races of very different physical types. The negroid contributions, at least in the Southern zone of this Late Quaternary field, must not be underestimated. The early steatopygous images—such as some of these of the Balzi Rossi caves—may safely be regarded as due to this ethnic type, which is also pictorially represented in some of the Spanish rock-paintings.

The nascent flame of primeval culture was thus already kindled in that Older World, and, so far as our present knowledge goes, it was in the South-Western part of our Continent, on either side of the Pyrenees, that it shone its brightest. After the great strides in human progress already made at that remote epoch, it is hard, indeed, to understand what it was that still delayed the rise of European civilisation in its higher shape. Yet it had to wait for its fulfilment through many millennia. The gathering shadows thickened and the darkness of a long night fell not on that favoured region alone, but throughout the wide area where Reindeer Man had ranged. Still the question rises—as yet imperfectly answered—were there no relay runners to pass on elsewhere the lighted torch?

Something, indeed, has been recently done towards bridging over the 'hiatus' that formerly separated the Neolithic from the Palæolithic Age—the yawning gulf between two Worlds of human existence. The Azilian—a later decadent outgrowth of the preceding culture—which is now seen partially to fill the lacuna, seems to be in some respects an impoverished survival of the Aurignacian.[6] The existence of this phase was first established by the long and patient investigations of Piette in the stratified deposits of the Cave of Mas d'Azil in the Ariège, from which it derives its name, and it has been proved by recent discoveries to have had a wide extension. It affords evidence of a milder and moister climate—well illustrated by the abundance of the little wood snail (*helix nemoralis*) and the increasing tendency of the Reindeer to die out in the Southern parts of the area, so that in the fabric of the characteristic harpoons deer-horns are used as substitutes. Artistic designs now fail us, but the polychrome technique of the preceding Age still survives in certain schematic and geometric figures, and in curious coloured signs on pebbles. These last first came to light in the Cave of Mas d'Azil, but they have now been found to recur much further afield in a similar association in grottoes from the neighbourhood of Basel to that of Salamanca. So like letters are some of these signs that the lively

[6] Breuil, *Congr. Préhist.* Geneva, 1912, p. 216.

imagination of Piette saw in them the actual characters of a primeval alphabet!

The little flakes with a worked edge often known as 'pygmy flints,' which were most of them designed for insertion into bone or horn harpoons, like some Neolithic examples, are very characteristic of this stratum, which is widely diffused in France and elsewhere under the misleading name of 'Tardenoisian.' At Ofnet, in Bavaria, it is associated with a ceremonial skull burial showing the coexistence at that spot of brachycephalic and dolichocephalic types, both of a new character. In Britain, as we know, this Azilian, or a closely allied phase, is traceable as far North as the Oban Caves.

What, however, is of special interest is the existence of a northern parallel to this cultural phase, first ascertained by the Danish investigator, Dr. Sarauw, in the Lake station of Maglemose, near the West coast of Zealand. Here bone harpoons of the Azilian type occur, with bone and horn implements showing geometrical and rude animal engravings of a character divergent from the Magdalenian tradition. The settlement took place when what is now the Baltic was still the great 'Ancylus Lake,' and the waters of the North Sea had not yet burst into it. It belongs to the period of the Danish pine and birch woods, and is shown to be anterior to the earliest shell mounds of the Kitchen-midden People, when the pine and the birch had given place to the oak. Similar deposits extend to Sweden and Norway, and to the Baltic Provinces as far as the Gulf of Finland. The parallel relationship of this culture is clear, and its remains are often accompanied with the characteristic 'pygmy' flints. Breuil, however,[7] while admitting the late Palæolithic character of this northern branch, would bring it into relation with a vast Siberian and Altaic province, distinguished by the widespread existence of rock-carvings of animals. It is interesting to note that a rock-engraving of a reindeer, very well stylised, from the Trondhjem Fjord, which has been referred to the Maglemosian phase, preserves the simple profile rendering—two legs only being visible—of Early Aurignacian tradition.

It is worth noting that an art affiliated to that of the petroglyphs of the old Altaic region long survived in the figures of the Lapp troll-drums, and still occasionally lingers, as I have myself had occasion to observe, on the reindeer-horn spoons of the Finnish and Russian Lapps, whose ethnic relationship, moreover, points east of the Ural. The existence of a Late Palæolithic Province on the Russian side is in any case now well recognised and itself supports the idea of a later shifting North and North-East, just as at a former period

[7] 'Les subdivisions du paléolithique supérieur et leur signification.'—*Congrès intern. d'Anthrop. et d'Archéol. préhist.*, XIVme Sess., Genève, 1912, pp. 165, 238.

it had oscillated in a South-Western direction. All this must be regarded as corroborating the view long ago expressed by Boyd Dawkins[8] that some part of the old Cave race may still be represented by the modern Eskimos. Testut's comparison of the short-statured Magdalenian skeleton from the rock shelter of Chancelade in the Dordogne with that of an Eskimo certainly confirms this conclusion.

On the other hand, the evidence, already referred to, of an extension of the Late Palæolithic culture to a North African zone, including rock-sculptures depicting a series of animals extinct there in the later Age, may be taken to favour the idea of a partial continuation on that side. Some of the early rock-sculptures in the south of the continent, such as the figure of a walking elephant reproduced by Dr. Peringuey, afford the clearest existing parallels to the best Magdalenian examples. There is much, indeed, to be said for the view, of which Sollas is an exponent, that the Bushmen, who at a more recent date entered that region from the North, and whose rock-painting attained such a high level of naturalist art, may themselves be taken as later representatives of the same tradition. In their human figures the resemblances descend even to conventional details, such as we meet with at Cogul and Alpera. Once more, we must never lose sight of the fact that from the Early Aurignacian Period onwards a negroid element in the broadest sense of the word shared in this artistic culture as seen on both sides of the Pyrenees.

At least we now know that Cave Man did not suffer any sudden extinction, though on the European side, partly, perhaps, owing to the new climatic conditions, this culture underwent a marked degeneration. It may well be that, as the osteological evidence seems to imply, some outgrowth of the old Cro-Magnon type actually perpetuated itself in the Dordogne. We have certainly lengthened our knowledge of the Palæolithic. But in the present state of the evidence it seems better to subscribe to Cartailhac's view that its junction with the Neolithic has not yet been reached. There does not seem to be any real continuity between the culture revealed at Maglemose and that of the immediately superposed Early Neolithic stratum of the shell-mounds, which, moreover, as has been already said, evidence a change both in climatic and geological conditions, implying a considerable interval of time.

It is a commonplace of Archæology that the culture of the Neolithic peoples throughout a large part of Central, Northern, and Western Europe—like the newly domesticated species possessed by them—is Eurasiatic in type. So, too, in Southern Greece and the Ægean World we meet with a form of Neolithic culture which must be essen-

[8] *Early Man in Britain*, 1880, p. 233 *seqq.*

tially regarded as a prolongation of that of Asia Minor.

It is clear that it is on this Neolithic foundation that our later civilisation immediately stands. But in the constant chain of actions and reactions by which the history of mankind is bound together—short of the extinction of all concerned, a hypothesis in this case excluded—it is equally certain that no great human achievement is without its continuous effect. The more we realise the substantial amount of progress of the men of the Late Quaternary Age in arts and crafts and ideas, the more difficult it is to avoid the conclusion that somewhere 'at the back of behind'—it may be by more than one route and on more than one continent, in Asia as well as Africa—actual links of connexion may eventually come to light.

Of the origins of our complex European culture this much at least can be confidently stated: the earliest extraneous sources on which it drew lay respectively in two directions—in the Valley of the Nile on one side and in that of the Euphrates on the other.

Of the high early culture in the lower Euphrates Valley our first real knowledge has been due to the excavations of De Sarzec in the Mounds of Tello, the ancient Lagash. It is now seen that the civilisation that we call Babylonian, and which was hitherto known under its Semitic guise, was really in its main features an inheritance from the earlier Sumerian race—culture in this case once more dominating nationality. Even the laws which Hammurabi traditionally received from the Babylonian Sun God were largely modelled on the reforms enacted a thousand years earlier by his predecessor, Urukagina, and ascribed by him to the inspiration of the City God of Lagash.[10] It is hardly necessary to insist on the later indebtedness of our civilisation to this culture in its Semitised shape, as passed on, together with other more purely Semitic elements, to the Mediterranean World through Syria, Canaan, and Phœnicia, or by way of Assyria, and by means of the increasing hold gained on the old Hittite region of Anatolia.

Even beyond the ancient Mesopotamian region which was the focus of these influences, the researches of De Morgan, Gautier, and Lampre. of the French 'Délégation en Perse,' have opened up another independent field, revealing a nascent civilisation equally ancient, of which Elam—the later Susiana—was the centre. Still further afield, moreover—some three hundred miles east of the Caspian—the interesting investigations of the Pumpelly Expedition in the mounds of Anau, near Ashkabad in Southern Turkestan, have brought to light a parallel and related culture. The painted Neolithic sherds of Anau, with their geometrical decoration, similar to contemporary ware of Elam, have suggested wide comparisons with the painted pottery of somewhat later date found in Cappadocia and other parts of Anatolia, as well as in

[10] See L. W. King, *History of Sumer and Akkad*, p. 184.

the North Syrian regions. It has, moreover, been reasonably asked whether another class of painted Neolithic fabrics, the traces of which extend across the Steppes of Southern Russia, and, by way of that ancient zone of migration, to the lower Danube and Northern Greece, may not stand in some original relation to the same ancient Province. The new discoveries, however, in the mounds of Elam and Anau have at most a bearing on the primitive phase of culture in parts of South-Eastern Europe that preceded the age when metal was generally in use.

Turning to the Nile Valley we are again confronted with an extraordinary revolution in the whole point of view effected during recent years. Thanks mainly to the methodical researches initiated by Flinders Petrie, we are able to look back beyond the Dynasties to the very beginnings of Egyptian civilisation. Already by the closing phase of the Neolithic and by the days of the first incipient use of metals the indigenous population had attained an extraordinarily high level. If on the one hand it displays Libyan connexions, on the other we already note the evidences of commercial intercourse with the Red Sea; and the constant appearance of large rowing vessels in the figured designs shows that the Nile itself was extensively used for navigation. Flint-working was carried to unrivalled perfection, and special artistic refinement was displayed in the manufacture of vessels of variegated breccia and other stones. The antecedent stages of many Egyptian hieroglyphs are already traceable, and the cult of Egyptian divinities, like Min, was already practised. Whatever ethnic change may have marked the establishment of Pharaonic rule, here, too, the salient features of the old indigenous culture were taken over by the new régime. This early Dynastic period itself has also received entirely new illustration from the same researches, and the freshness and force of its artistic works in many respects outshine anything produced in the later course of Egyptian history.

The continuity of human tradition as a whole in areas geographically connected like Eurafrica on the one side and Eurasia on the other has been here postulated. Since, as we have seen, the Late Palæolithic culture was not violently extinguished but shows signs of survival both North and South, we are entitled to trace elements of direct derivation from this source among the inherited acquirements that finally led up to the higher forms of ancient civilisation that arose on the Nile and the Euphrates. In many directions, we may believe, the flaming torch had been carried on by the relay runners.

But what, it may be asked, of Greece itself, where human culture reached its highest pinnacle in the Ancient World and to which we look as the principal source of our own civilisation?

Till within recent years it seemed almost a point of honour for classical scholars to regard Hellenic civilisation as a Wonder-Child,

sprung, like Athena herself, fully panoplied from the head of Zeus. The indebtedness to Oriental sources was either regarded as comparatively late or confined to such definite borrowings as the alphabet or certain weights and measures. Egypt, on the other hand, at least till Alexandrine times, was looked on as something apart, and it must be said that Egyptologists on their side were only too anxious to preserve their sanctum from profane contact.

A truer perspective has now been opened out. It has been made abundantly clear that the rise of Hellenic civilisation was itself part of a wider economy and can be no longer regarded as an isolated phenomenon. Indirectly, its relation to the greater World and to the ancient centres to the South and East has been now established by its affiliation to the civilisation of prehistoric Crete and by the revelation of the extraordinarily high degree of proficiency that was there attained in almost all departments of human art and industry. That Crete itself—the 'Mid-Sea land,' a kind of halfway house between three continents—should have been the cradle of our European civilisation was, in fact, a logical consequence of its geographical position. An outlier of Mainland Greece, almost opposite the mouths of the Nile, primitive intercourse between Crete and the further shores of the Libyan Sea was still further facilitated by favourable winds and currents. In the Eastern direction, on the other hand, island stepping-stones brought it into easy communication with the coast of Asia Minor, with which it was actually connected in late geological times.

But the extraneous influences that were here operative from a remote period encountered on the island itself a primitive indigenous culture that had grown up there from immemorial time. In view of some recent geological calculations, such as those of Baron De Geer, who by counting the number of layers of mud in Lake Ragunda has reduced the ice-free period in Sweden to 7,000 years, it will not be superfluous to emphasise the extreme antiquity that seems to be indicated for even the later Neolithic in Crete. The Hill of Knossos, upon which the remains of the brilliant Minoan civilisation have found their most striking revelation, itself resembles in a large part of its composition a great mound or Tell—like those of Mesopotamia or Egypt—formed of layer after layer of human deposits. But the remains of the whole of the later Ages represented down to the earliest Minoan period (which itself goes back to a time contemporary with the early Dynasties of Egypt—at a moderate estimate to 3400 B.C.) occupy considerably less than a half—19 feet, that is, out of a total of over 45. Such calculations can have only a relative value, but, even if we assume a more rapid accumulation of débris for the Neolithic strata and deduct a third from our calculation, they would still occupy a space of over 3,400 years, giving a total antiquity of some 9,000 years from the present

time.[11] No Neolithic section in Europe can compare in extent with that of Knossos, which itself can be divided by the character of its contents into an Early, Middle, and Late phase. But its earliest stratum already shows the culture in an advanced stage, with carefully ground and polished axes and finely burnished pottery. The beginnings of Cretan Neolithic must go back to a still more remote antiquity.

The continuous history of the Neolithic Age is carried back at Knossos to an earlier epoch than is represented in the deposits of its geographically related areas on the Greek and Anatolian side. But sufficient materials for comparison exist to show that the Cretan branch belongs to a vast Province of primitive culture that extended from Southern Greece and the Ægean islands throughout a wide region of Asia Minor and probably still further afield.

An interesting characteristic is the appearance in the Knossian deposits of clay images of squatting female figures of a pronouncedly steatopygous conformation and with hands on the breasts. These in turn fit on to a large family of similar images which recur throughout the above area, though elsewhere they are generally known in their somewhat developed stage, showing a tendency to be translated into stone, and finally—perhaps under extraneous influences both from the North and East—taking a more extended attitude. These clearly stand in a parallel relationship to a whole family of figures with the organs of maternity strongly developed that characterise the Semitic lands and which seem to have spread from there to Sumeria and to the seats of the Anau culture.

At the same time this steatopygous family, which in other parts of the Mediterranean basin ranges from prehistoric Egypt and Malta to the North of Mainland Greece, calls up suggestive reminiscences of the similar images of Aurignacian Man. It is especially interesting to note that in Crete, as in the Anatolian region where these primitive images occur, the worship of a Mother Goddess predominated in later times, generally associated with a divine Child—a worship which later survived in a classical guise and influenced all later religion. Another interesting evidence of the underlying religious community between Crete and Asia Minor is the diffusion in both areas of the cult of the Double Axe. This divine symbol, indeed, or 'Labrys,' became the special emblem of the Palace sanctuary of Knossos itself, which owes to it its traditional name of Labyrinth. I have already called attention to the fact that the absorptive and disseminating power of the Roman Empire brought the cult of a male form of the divinity of the Double Axe to the Roman Wall and to the actual site on which Newcastle stands.

The fact should never be left out of sight that the gifted indigenous

[11] For a fuller statement I must refer to my forthcoming work, *The Nine Minoan Periods* (Macmillans), Vol. I.: Neolithic Section.

stock which in Crete eventually took to itself on one hand and the other so many elements of exotic culture was still deep-rooted in its own. It had, moreover, the advantages of an insular people in taking what it wanted and no more. Thus it was stimulated by foreign influences but never dominated by them, and there is nothing here of the servility of Phœnician art. Much as it assimilated, it never lost its independent tradition.

It is interesting to note that the first quickening impulse came to Crete from the Egyptian and not from the Oriental side—the Eastern factor, indeed, is of comparatively late appearance. My own researches have led me to the definite conclusion that cultural influences were already reaching Crete from beyond the Libyan Sea before the beginning of the Egyptian Dynasties. These primitive influences are attested, amongst other evidences, by the forms of stone vessels, by the same æsthetic tradition in the selection of materials distinguished by their polychromy, by the appearance of certain symbolic signs, and the subjects of shapes and seals which go back to prototypes in use among the 'Old Race' of the Nile Valley. The impression of a very active agency indeed is so strong that the possibility of some actual immigration into the island of the older Egyptian element, due to the conquests of the first Pharaohs, cannot be excluded.

The continuous influence of Dynastic Egypt from its earliest period onwards is attested both by objects of import and their indigenous imitations, and an actual monument of a Middle Empire Egyptian was found in the Palace Court at Knossos. More surprising still are the cumulative proofs of the reaction of this early Cretan civilisation on Egypt itself, as seen not only in the introduction there of such beautiful Minoan fabrics as the elegant polychrome vases, but in the actual impress observable on Egyptian Art even on its religious side. The Egyptian griffin is fitted with Minoan wings. So, too, on the other side we see the symbols of Egyptian religion impressed into the service of the Cretan Nature Goddess, who in certain respects was partly assimilated with Hathor, the Egyptian Cow-Goddess of the Underworld.

My own most recent investigations have more and more brought home to me the all-pervading community between Minoan Crete and the land of the Pharaohs. When we realise the great indebtedness of the succeeding classical culture of Greece to its Minoan predecessor the full significance of this conclusion will be understood. Ancient Egypt itself can no longer be regarded as something apart from general human history. Its influences are seen to lie about the very cradle of our own civilisation.

The high early culture, the equal rival of that of Egypt and Babylonia, which thus began to take its rise in Crete in the fourth millennium

before our era, flourished for some two thousand years, eventually dominating the Ægean and a large part of the Mediterranean basin. To the civilisation as a whole I ventured, from the name of the legendary King and [illegible] of Crete, to apply the name of 'Minoan,' which has received general acceptance; and it has been possible now to divide its course into three Ages—Early, Middle, and Late, answering roughly to the successive Egyptian Kingdoms, and each in turn with a triple subdivision.

It is difficult indeed in a few words to do adequate justice to this earliest of European civilisations. Its achievements are too manifold. The many-storeyed palaces of the Minoan priest-kings in their great days, by their ingenious planning, their successful combination of the useful with the beautiful and stately, and, last but not least, by their scientific sanitary arrangements, far outdid the similar works, on however vast a scale, of Egyptian or Babylonian builders. What is more, the same skilful and commodious construction recurs in a whole series of private mansions and smaller dwellings throughout the island. Outside 'broad Knossos' itself, flourishing towns sprang up far and wide on the country sides. New and refined crafts were developed, some of them, like that of the inlaid metal-work, unsurpassed in any age or country. Artistic skill, of course, reached its acme in the great palaces themselves, the corridors, landings, and porticoes of which were decked with wall paintings and high reliefs, showing in the treatment of animal life not only an extraordinary grasp of Nature, but a grandiose power of composition such as the world had never seen before. Such were the great bull-grappling reliefs of the Sea Gate at Knossos and the agonistic scenes of the great Palace hall.

The modernness of much of the life here revealed to us is astonishing. The elaboration of the domestic arrangements, the staircases storey above storey, the front places given to the ladies at shows, their fashionable flounced robes and jackets, the gloves sometimes seen on their hands or hanging from their folding chairs, their very mannerisms as seen on the frescoes, pointing their conversation with animated gestures—how strangely out of place would it all appear in a classical design! Nowhere, not even at Pompeii, have more living pictures of ancient life been called up for us than in the Minoan Palace of Knossos. The touches supplied by its closing scene are singularly dramatic—the little bath-room opening out of the Queen's parlour, with its painted clay bath, the royal draught-board flung down in the court, the vessels for anointing and the oil-jar for their filling ready to hand by the throne of the Priest-King, with the benches of his Consistory round and the sacral griffins on either side. Religion, indeed, entered in at every turn. The palaces were also temples, the tomb a shrine of the Great Mother. It was perhaps owing to the

religious control of art that among all the Minoan representations—now to be numbered by thousands—no single example of indecency has come to light.

A remarkable feature of this Minoan civilisation cannot be passed over. I remember that at the Liverpool Meeting of this Association in 1896—just before the first results of the new discoveries in Crete were known—a distinguished archæologist took as the subject of an evening lecture 'Man before Writing,' and, as a striking example of a high culture attained by '*Analfabeti*,' singled out that of Mycenæ—a late offshoot, as we know now, from Minoan Crete. To such a conclusion, based on negative evidence, I confess I could never subscribe—for had not even the people of the Reindeer Age attained to a considerable proficiency in expression by means of symbolic signs? To-day we are able to trace the gradual evolution on Cretan soil of a complete system of writing from its earliest pictographic shape, through a conventionalised hieroglyphic to a linear stage of great perfection. In addition to inscribed sealings and other records some two thousand clay tablets have now come to light, mostly inventories or contracts; for though the script itself is still undeciphered the pictorial figures that often appear on these documents supply a valuable clue to their contents. The numeration also is clear, with figures representing sums up to 10,000. The inscribed sealings, signed, counter-marked, and counter-signed by controlling officials, give a high idea of the elaborate machinery of Government and Administration under the Minoan rulers.

The minutely organised legal conditions to which this points confirm the later traditions of Minos, the great law-giver of prehistoric Crete, who, like Hammurabi and Moses, was said to have received the law from the God of the Sacred Mountain. The clay tablets themselves were certainly due to Oriental influences, which make themselves perceptible in Crete at the beginning of the Late Minoan Age, and may have been partly resultant from the reflex action of Minoan colonisation in Cyprus. From this time onwards Eastern elements are more and more traceable in Cretan culture, and are evidenced by such phenomena as the introduction of chariots—themselves perhaps more remotely of Aryan-Iranian derivation—and by the occasional use of cylinder seals.

Simultaneously with its Eastern expansion, which affected the coast of Phœnicia and Palestine as well as Cyprus, Minoan civilisation now took firm hold of Mainland Greece, while traces of its direct influence are found in the West Mediterranean basin—in Sicily, the Balearic Islands, and Spain. At the time of the actual Conquest and during the immediately succeeding period the civilisation that appears at Mycenæ and Tiryns, at Thebes and Orchomenos, and at other centres of Mainland Greece, though it seems to have brought with it some already assimilated Anatolian elements, is still in the broadest sense

Minoan. It is only at a later stage that a more provincial offshoot came into being to which the name Mycenæan can be properly applied. But it is clear that some vanguard at least of the Aryan Greek immigrants came into contact with this high Minoan culture at a time when it was still in its most flourishing condition. The evidence of Homer itself is conclusive. Arms and armour described in the poems are those of the Minoan prime, the fabled shield of Achilles, like that of Herakles described by Hesiod, with its elaborate scenes and variegated metal-work, reflects the masterpieces of Minoan craftsmen in the full vigour of their art; the very episodes of epic combat receive their best illustration on the signets of the great days of Mycenæ. Even the lyre to which the minstrel sang was a Minoan invention. Or, if we turn to the side of religion, the Greek temple seems to have sprung from a Minoan hall, its earliest pediment schemes are adaptations from the Minoan tympanum—such as we see in the Lions' Gate—the most archaic figures of the Hellenic Goddesses, like the Spartan Orthia, have the attributes and attendant animals of the great Minoan Mother.

Some elements of the old culture were taken over on the soil of Hellas. Others which had been crushed out in their old centres survived in the more Eastern shores and islands formerly dominated by Minoan civilisation, and were carried back by Phœnician or Ionian intermediaries to their old homes. In spite of the overthrow which about the twelfth century before our era fell on the old Minoan dominion and the onrush of the new conquerors from the North, much of the old tradition still survived to form the base for the fabric of the later civilisation of Greece. Once more, through the darkness, the lighted torch was carried on, the first glimmering flame of which had been painfully kindled by the old Cave dwellers in that earlier Palæolithic World.

The Roman Empire, which in turn appropriated the heritage that Greece had received from Minoan Crete, placed civilisation on a broader basis by welding together heterogeneous ingredients and promoting a cosmopolitan ideal. If even the primeval culture of the Reindeer Age embraced more than one race and absorbed extraneous elements from many sides, how much more is that the case with our own which grew out of the Greco-Roman! Civilisation in its higher form to-day, though highly complex, forms esentially a unitary mass. It has no longer to be sought out in separate luminous centres, shining like planets through the surrounding night. Still less is it the property of one privileged country or people. Many as are the tongues of mortal men, its votaries, like the Immortals, speak a single language. Throughout the whole vast area illumined by its quickening rays, its workers are interdependent, and pledged to a common cause.

We, indeed, who are met here to-day to promote in a special way

the Cause of Truth and Knowledge, have never had a more austere duty set before us. I know that our ranks are thinned. How many of those who would otherwise be engaged in progressive research have been called away for their country's service! How many who could least be spared were called to return no more! Scientific intercourse is broken, and its cosmopolitan character is obscured by the death struggle in which whole Continents are locked. The concentration, moreover, of the Nation and of its Government on immediate ends has distracted it from the urgent reforms called for by the very evils that are the root cause of many of the greatest difficulties it has had to overcome. It is a lamentable fact that beyond any nation of the West the bulk of our people remains sunk not in comparative ignorance only—for that is less difficult to overcome—but in intellectual apathy. The dull incuria of the parents is reflected in the children, and the desire for the acquirement of knowledge in our schools and colleges is appreciably less than elsewhere. So, too, with the scientific side of education, it is not so much the actual amount of Science taught that is in question—insufficient as that is—as the instillation of the scientific spirit itself—the perception of method, the sacred thirst for investigation.

But can we yet despair of the educational future of a people that has risen to the full height of the great emergency with which they were confronted? Can we doubt that, out of the crucible of fiery trial, a New England is already in the moulding?

We must all bow before the hard necessity of the moment. Of much we cannot judge. Great patience is demanded. But let us, who still have the opportunity of doing so, at least prepare for the even more serious struggle that must ensue against the enemy in our midst, that [illegible]. We have to deal with ignorance, apathy, the non-scientific mental attitude, the absorption of popular interest in sports and amusements.

And what, meanwhile, is the attitude of those in power—of our Government, still more of our permanent officials? A cheap epigram is worn threadbare in order to justify the ingrained distrust of expert, in other words of scientific, advice on the part of our public offices. We hear, indeed, of 'Commissions' and 'Enquiries,' but the inveterate attitude of our rulers towards the higher interests that we are here to promote is too clearly shown by a single episode. It is those higher interests that are the first to be thrown to the wolves. All are agreed that special treasures should be stored in positions of safety, but at a time when it might have been thought desirable to keep open every avenue of popular instruction and of intelligent diversion, the galleries of our National Museum at Bloomsbury were entirely closed for the sake of the paltriest saving—three minutes, it was calculated, of the cost of the

War to the British Treasury! That some, indeed, were left open elsewhere was not so much due to the enlightened sympathy of our politicians, as to their alarmed interests in view of the volume of intelligent protest. Our friends and neighbours across the Channel, under incomparably greater stress, have acted in a very different spirit.

It will be a hard struggle for the friends of Science and Education, and the air is thick with mephitic vapours. Perhaps the worst economy to which we are to-day reduced by our former lack of preparedness is the economy of Truth. Heaven knows!—it may be a necessary penalty. But its results are evil. Vital facts that concern our national well-being, others that even affect the cause of a lasting Peace, are constantly suppressed by official action. The negative character of the process at work which conceals its operation from the masses makes it the more insidious. We live in a murky atmosphere amidst the suggestion of the false, and there seems to be a real danger that the recognition of Truth as itself a Tower of Strength may suffer an eclipse.

It is at such a time and under these adverse conditions that we, whose object it is to promote the Advancement of Science, are called upon to act. It is for us to see to it that the lighted torch handed down to us from the Ages shall be passed on with a still brighter flame. Let us champion the cause of Education, in the best sense of the word, as having regard to its spiritual as well as its scientific side. Let us go forward with our own tasks, unflinchingly seeking for the Truth, confident that, in the eternal dispensation, each successive generation of seekers may approach nearer to the goal.

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British Association for the Advancement of Science.

CARDIFF, 1920.

ADDRESS

BY

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PRESIDENT.

Oceanography and the Sea-Fisheries.

It has been customary, when occasion required, for the President to offer a brief tribute to the memory of distinguished members of the Association lost to Science during the preceding year. These, for the most part, have been men of advanced years and high reputation, who had completed their life-work and served well in their day the Association and the sciences which it represents. We have this year no such losses to record. But it seems fitting on the present occasion to pause for a moment and devote a grateful thought to that glorious band of fine young men of high promise in science who, in the years since our Australian meeting in 1914, gave, it may be, in brief days and months of sacrifice, greater service to humanity and the advance of civilisation than would have been possible in years of normal time and work. A few names stand out already known and highly honoured—Moseley, Jenkinson, Geoffrey Smith, Keith Lucas, Gregory, and more recently Leonard Doncaster—all grievous losses; but there are also others, younger members of our Association, who had not yet had opportunity for showing accomplished work, but who equally gave up all for a great ideal. I prefer to offer a collective rather than an individual tribute. Other young men of science will arise and carry on their work—but the gap in our ranks remains. Let their successors remember that it serves as a reminder of a great example and of high endeavour worthy of our gratitude and of permanent record in the annals of Science.

At the last Cardiff Meeting of the British Association in 1891 you had as your President the eminent astronomer Sir William Huggins, who discoursed upon the then recent discoveries of the spectroscope in relation to the chemical nature, density, temperature, pressure and even the motions of the stars. From the sky to the sea is a long drop; but the sciences of both have this in common that they deal with

fundamental principles and with vast numbers. Over three hundred years ago Spenser in the 'Faerie Queene' compared 'the seas abundant progeny' with 'the starres on hy,' and recent investigations show that a litre of sea-water may contain more than a hundred times as many living organisms as there are stars visible to the eye on a clear night.

During the past quarter of a century great advances have been made in the science of the sea, and the aspects and prospects of sea-fisheries research have undergone changes which encourage the hope that a combination of the work now carried on by hydrographers and biologists in most civilised countries on fundamental problems of the ocean may result in a more rational exploitation and administration of the fishing industries.

And yet even at your former Cardiff Meeting thirty years ago there were at least three papers of oceanographic interest—one by Professor Osborne Reynolds on the action of waves and currents, another by Dr. H. R. Mill on seasonal variation in the temperature of lochs and estuaries, and the third by our Honorary Local Secretary for the present meeting, Dr. Evans Hoyle, on a deep-sea tow-net capable of being opened and closed under water by the electric current.

It was a notable meeting in several other respects, of which I shall merely mention two. In Section A, Sir Oliver Lodge gave the historic address in which he expounded the urgent need, in the interests of both science and the industries, of a national institution for the promotion of physical research on a large scale. Lodge's pregnant idea put forward at this Cardiff Meeting, supported and still further elaborated by Sir Douglas Galton as President of the Association at Ipswich, has since borne notable fruit in the establishment and rapid development of the National Physical Laboratory. The other outstanding event of that meeting is that you then appointed a committee of eminent geologists and naturalists to consider a project for boring through a coral reef, and that led during following years to the successive expeditions to the atoll of Funafuti in the Central Pacific, the results of which, reported upon eventually by the Royal Society, were of great interest alike to geologists, biologists, and oceanographers.

Dr. Huggins, on taking the Chair in 1891, remarked that it was over thirty years since the Association had honoured Astronomy in the selection of its President. It might be said that the case of Oceanography is harder, as the Association has never had an Oceanographer as President—and the Association might well reply 'Because until very recent years there has been no Oceanographer to have.' If Astronomy is the oldest of the sciences, Oceanography is probably the youngest. Depending as it does upon the methods and results of other sciences, it was not until our knowledge of Physics, Chemistry, and Biology were

relatively far advanced that it became possible to apply that knowledge to the investigation and explanation of the phenomena of the ocean. No one man has done more to apply such knowledge derived from various other subjects and to organise the results as a definite branch of science than the late Sir John Murray, who may therefore be regarded as the founder of modern Oceanography.

It is, to me, a matter of regret that Sir John Murray was never President of the British Association. I am revealing no secret when I tell you that he might have been. On more than one occasion he was invited by the Council to accept nomination and he declined for reasons that were good and commanded our respect. He felt that the necessary duties of this post would interfere with what he regarded as his primary life-work—oceanographical explorations already planned, and the last of which he actually carried out in the North Atlantic in 1912, when over seventy years of age, in the Norwegian steamer *Michael Sars* along with his friend Dr. Johan Hjort.

Anyone considering the subject-matter of this new science must be struck by its wide range, overlapping as it does the borderlands of several other sciences and making use of their methods and facts in the solution of its problems. It is not only world-wide in its scope but extends beyond our globe and includes astronomical data in their relation to tidal and certain other oceanographical phenomena. No man in his work, or even thought, can attempt to cover the whole ground—although Sir John Murray, in his remarkably comprehensive 'Summary' volumes of the *Challenger* Expedition and other writings, went far towards doing so. He, in his combination of [illegible] chemist, geologist and biologist, was the nearest approach we have had to an all-round Oceanographer. The International Research Council probably acted wisely at the recent Brussels Conference in recommending the institution of two International Sections in our subject, the one of physical and the other of biological Oceanography—although the two overlap and are so interdependent that no investigator on the one side can afford to neglect the other.[1]

On the present occasion I must restrict myself almost wholly to the latter division of the subject, and be content, after brief reference to the

[1] The following classification of the primary divisions of the subject may possibly be found acceptable :—

Physiography

Oceanography — Geography

Oceanography:

Hydrography (Physics, &c.) — Metabolism (Bio-Chemistry) — Bionomics (Biology) — Tidology (Mathematics)

founders and pioneers of our science, to outline a few of those investigations and problems which have appeared to me to be of fundamental importance, of economic value, or of general interest.

Although the name Oceanography was only given to this branch of science by Sir John Murray in 1880, and although according to that veteran oceanographer Mr. J. Y. Buchanan, the last surviving member of the civilian staff of the *Challenger*, the science of Oceanography was born at sea on February 15, 1873,[2] when, at the first official dredging station of the expedition, to the westward of Teneriffe, at 1525 fathoms, everything that came up in the dredge was new and led to fundamental discoveries as to the deposits forming on the floor of the ocean, still it may be claimed that the foundations of the science were laid by various explorers of the ocean at much earlier dates. Aristotle, who took all knowledge for his province, was an early oceanographer on the shores of Asia Minor. When Pytheas passed between the pillars of Hercules into the unknown Atlantic and penetrated to British seas in the fourth century B.C., and brought back reports of Ultima Thule and of a sea to the North thick and sluggish like a jelly-fish, he may have been recording an early planktonic observation. But passing over all such and many other early records of phenomena of the sea, we come to surer ground in claiming, as founders of Oceanography, Count Marsili, an early investigator of the Mediterranean, and that truly scientific navigator Captain James Cook, who sailed to the South Pacific on a Transit of Venus expedition in 1769 with Sir Joseph Banks as naturalist, and by subsequently circumnavigating the South Sea about latitude 60° finally disproved the existence of a great southern continent; and Sir James Clerk Ross, who, with Sir Joseph Hooker as naturalist, first dredged the Antarctic in 1840.

The use of the naturalist's dredge (introduced by O. F. Müller, the Dane, in 1799) for exploring the sea-bottom was brought into prominence almost simultaneously in several countries of North-West Europe —by Henri Milne-Edwards in France in 1830, Michael Sars in Norway in 1835, and our own Edward Forbes about 1832.

The last-mentioned genial and many-sided genius was a notable figure in several sections of the British Association from about 1836 onwards, and may fairly be claimed as a pioneer of Oceanography. In 1839 he and his friend the anatomist, John Goodsir, were dredging

[2] Others might put the date later. Significant [illegible] are Sir John [illegible] Summary Volumes of the *Challenger* (1895), the inauguration of [illegible] O[illegible]' at Monaco in 1910, the foundation of the 'Institut [illegible] at Paris in 1906 (see the Prince of Monaco's letter to the Minister of Public Instruction), and Sir John Murray's little book 'The Ocean' (1913), where the superiority of the term Oceanography to Thalassography (used by Alexander Agassiz) is discussed.

in the Shetland seas, with results which Forbes made known to the meeting of the British Association at Birmingham that summer, with such good effect that a 'Dredging Committee'[3] of the Association was formed to continue the good work. Valuable reports on the discoveries of that Committee appear in our volumes at intervals during the subsequent twenty-five years.

It has happened over and over again in history that the British Association, by means of one of its research committees, has led the way in some important new research or development of science and has shown the Government or an industry what wants doing and how it can be done. We may fairly claim that the British Association has inspired and fostered that exploration of British seas which through marine biological investigations and deep-sea expeditions has led on to modern Oceanography. Edward Forbes and the British Association Dredging Committee, Wyville Thomson, Carpenter, Gwyn Jeffreys, Norman and other naturalists of the pre-*Challenger* days—all these men in the quarter-century from 1840 onwards worked under research committees of the British Association, bringing their results before successive meetings; and some of our older volumes enshrine classic reports on dredging by Forbes, McAndrew, Norman, Brady, Alder, and other notable naturalists of that day. These local researches paved the way for the *Challenger* and other national deep-sea expeditions. Here, as in other cases, it required private enterprise to precede and stimulate Government action.

It is probable that Forbes and his fellow-workers on this 'Dredging Committee' in their marine explorations did not fully realise that they were opening up a most comprehensive and important department of knowledge. But it is also true that in all his expeditions—in the British seas from the Channel Islands to the Shetlands, in Norway, in the Mediterranean as far as the Ægean Sea—his broad outlook on the problems of nature was that of the modern oceanographer, and he was the spiritual ancestor of men like Sir Wyville Thomson of the *Challenger* Expedition and Sir John Murray, whose accidental death a few years ago, while still in the midst of active work, was a grievous loss to this new and rapidly advancing science of the sea.

Forbes in these marine investigations worked at border-line problems, dealing for example with the relations of Geology to Zoology.

[3] 'For researches with the dredge, with a view to the investigation of the marine zoology of Great Britain, the illustration of the geographical distribution of marine animals, and the more accurate [illegible] of the fossils of the pleistocene period: under the superintendence of Mr. Gray, Mr. Forbes, Mr. Goodsir, Mr. Patterson, Mr. Thompson of Belfast, Mr. Ball of Dublin, Dr. George Johnston, Mr. Smith of Jordan Hill, and Mr. A. Strickland, £60.' Report for 1839, p. xxvi.

and the effect of the past history of the land and sea upon the distribution of plants and animals at the present day, and in these respects he was an early oceanographer. For the essence of that new subject is that it also investigates border-line problems and is based upon and makes use of all the older fundamental sciences—Physics, Chemistry and Biology—and shows for example how variations in the great ocean currents may account for the movements and abundance of the migratory fishes, and how periodic changes in the physico-chemical characters of the sea, such as variations in the hydrogen-ion and hydroxyl-ion concentration, are correlated with the distribution at the different seasons of the all-important microscopic organisms that render our oceanic waters as prolific a source of food as the pastures of the land.

Another pioneer of the nineteenth century who, I sometimes think, has not yet received sufficient credit for his foresight and initiative, is Sir Wyville Thomson, whose name ought to go down through the ages as the leader of the scientific staff on the famous *Challenger* Deep-Sea Exploring Expedition. It is due chiefly to him and to his friend Dr. W. B. Carpenter that the British Government, through the influence of the Royal Society, was induced to place at the disposal of a committee of scientific experts first the small surveying steamer *Lightning* in 1868, and then the more efficient steamer *Porcupine* in the two succeeding years, for the purpose of exploring the deep water of the Atlantic from the Faroes in the North to Gibraltar and beyond in the South, in the course of which expeditions they got successful hauls from the then unprecedented depth of 2435 fathoms, nearly three statute miles.

It will be remembered that Edward Forbes, from his observations in the Mediterranean (an abnormal sea in some respects), regarded depths of over 300 fathoms as an azoic zone. It was the work of Wyville Thomson and his colleagues Carpenter and Gwyn Jeffreys on these successive dredging expeditions to prove conclusively what was beginning to be suspected by naturalists, that there is no azoic zone in the sea, but that abundant life belonging to many groups of animals extends down to the greatest depths of from four to five thousand fathoms—nearly six statute miles from the surface.

These pioneering expeditions in the *Lightning* and *Porcupine*—the results of which are not even yet fully made known to science—were epoch-making, inasmuch as they not only opened up this new region to the systematic marine biologist, but gave glimpses of world-wide problems in connection with the physics, the chemistry and the biology of the sea which are only now being adequately investigated by the modern oceanographer. These results, which aroused intense interest amongst the leading scientific men of the time, were so rapidly surpassed and overshadowed by the still greater achievements of the

Challenger and other national exploring expeditions that followed in the 'seventies and 'eighties of last century, that there is some danger of their real importance being lost sight of; but it ought never to be forgotten that they first demonstrated the abundance of life of a varied nature in depths formerly supposed to be azoic, and, moreover, that some of the new deep-sea animals obtained were related to extinct forms belonging to the Jurassic, Cretaceous and Tertiary periods.

It is interesting to recall that our Association played its part in promoting the movement that led to the *Challenger* Expedition. Our General Committee at the Edinburgh Meeting of 1871 recommended that the President and Council be authorised to co-operate with the Royal Society in promoting 'a Circumnavigation Expedition, specially fitted out to carry the Physical and Biological Exploration of the Deep Sea into all the Great Oceanic Areas'; and our Council subsequently appointed a committee consisting of Dr. Carpenter, Professor Huxley and others to co-operate with the Royal Society in carrying out these objects.

It has been said that the *Challenger* Expedition will rank in history with the voyages of Vasco da Gama, Columbus, Magellan and Cook. Like these it added new regions of the globe to our knowledge, and the wide expanses thus opened up for the first time, the floors of the oceans, though less accessible, are vaster than the discoveries of any previous exploration.

Sir Wyville Thomson, although leader of the expedition, did not live to see the completed results, and Sir John Murray will be remembered in the history of science as the *Challenger* naturalist who brought to a successful issue the investigation of the enormous collections and the publication of the scientific results of that memorable voyage: these two Scots share the honour of having guided the destinies of what is still the greatest oceanographic exploration of all times.

In addition to taking his part in the general work of the expedition, Murray devoted special attention to three subjects of primary importance in the science of the sea, viz.: (1) the plankton or floating life of the oceans, (2) the deposits forming on the sea bottoms, and (3) the origin and mode of formation of coral reefs and islands. It was characteristic of his broad and synthetic outlook on nature that, in place of working at the speciography and anatomy of some group of organisms, however novel, interesting and attractive to the naturalist the deep-sea organisms might seem to be, he took up wide-reaching general problems with economic and geological as well as biological applications.

Each of the three main lines of investigation—deposits, plankton and coral reefs—which Murray undertook on board the *Challenger* has been most fruitful of results both in his own hands and those of

others. His plankton work has led on to those modern planktonic researches which are closely bound up with the scientific investigation of our sea-fisheries.

His work on the deposits accumulating on the floor of the ocean resulted, after years of study in the laboratory as well as in the field, in collaboration with the Abbé Renard of the Brussels Museum, afterwards Professor at Ghent, in the production of the monumental 'Deep-Sea Deposits' volume, one of the *Challenger* Reports, which first revealed to the scientific world the detailed nature and distribution of the varied submarine deposits of the globe and their relation to the rocks forming the crust of the earth.

These studies led, moreover, to one of the romances of science which deeply influenced Murray's future life and work. In accumulating material from all parts of the world and all deep-sea exploring expeditions for comparison with the *Challenger* series, some ten years later, Murray found that a sample of rock from Christmas Island in the Indian Ocean, which had been sent to him by Commander (now Admiral) Aldrich, of H.M.S. *Egeria*, was composed of a valuable phosphatic material. This discovery in Murray's hands gave rise to a profitable commercial undertaking, and he was able to show that some years ago the British Treasury had already received in royalties and taxes from the island considerably more than the total cost of the *Challenger* Expedition.

That first British circumnavigating expedition on the *Challenger* was followed by other national expeditions (the American *Tuscarora* and *Albatross*, the French *Travailleur*, the German *Gauss*, *National*, and *Valdivia*, the Italian *Vettor Pisani*, the Dutch *Siboga*, the Danish *Thor* and others) and by almost equally celebrated and important work by unofficial oceanographers such as Alexander Agassiz, Sir John Murray with Dr. Hjort in the *Michael Sars*, and the Prince of Monaco in his magnificent ocean-going yacht, and by much other good work by many investigators in smaller and humbler vessels. One of these supplementary expeditions I must refer to briefly because of its connection with sea-fisheries. The *Triton*, under Tizard and Murray, in 1882, while exploring the cold and warm areas of the Faroe Channel separated by the Wyville-Thomson ridge, incidentally discovered the famous Dubh-Artach fishing-grounds, which have been worked by British trawlers ever since.

Notwithstanding all this activity during the last forty years since Oceanography became a science, much has still to be investigated in all seas in all branches of the subject. On pursuing any line of investigation one very soon comes up against a wall of the unknown or a maze of controversy. Peculiar difficulties surround the subject. The

matters investigated are often remote and almost inaccessible. Unknown factors may enter into every problem. The samples required may be at the other end of a rope or a wire eight or ten miles long, and the oceanographer may have to grope for them literally in the dark and under other difficult conditions which make it uncertain whether his samples when obtained are adequate and representative, and whether they have undergone any change since leaving their natural environment. It is not surprising then that in the progress of knowledge mistakes have been made and corrected, that views have been held on what seemed good scientific grounds which later on were proved to be erroneous. For example, Edward Forbes, in his division of life in the sea into zones, on what then seemed to be sufficiently good observations in the Ægean, but which we now know to be exceptional, placed the limit of life at 300 fathoms, while Wyville Thomson and his fellow-workers on the *Porcupine* and the *Challenger* showed that there is no azoic zone even in the great abysses.

Or, again, take the celebrated myth of 'Bathybius.' In the 'sixties of last century samples of Atlantic mud, taken when surveying the bottom for the first telegraph cables and preserved in alcohol, were found when examined by Huxley, Haeckel and others to contain what seemed to be an exceedingly primitive protoplasmic organism, which was supposed on good evidence to be widely extended over the floor of the ocean. The discovery of this Bathybius was said to solve the problem of how the deep-sea animals were nourished in the absence of seaweeds. Here was a widespread protoplasmic meadow upon which other organisms could graze. Belief in Bathybius seemed to be confirmed and established by Wyville Thomson's results in the *Porcupine* Expedition of 1869, but was exploded by the naturalists on the *Challenger* some five years later. Buchanan in his recently published 'Accounts Rendered' tells us how he and his colleague Murray were keenly on the look-out for hours at a time on all possible occasions for traces of this organism, and how they finally proved, in the spring of 1875 on the voyage between Hong-Kong and Yokohama, that the all-pervading substance like coagulated mucus was an amorphous precipitate of sulphate of lime thrown down from the sea-water in the mud on the addition of a certain proportion of alcohol. He wrote to this effect from Japan to Professor Crum Brown, and it is in evidence that after receiving this letter Crum Brown interested his friends in Edinburgh by showing them how to make Bathybius in the chemical laboratory. Huxley at the Sheffield Meeting of the British Association in 1879 handsomely admitted that he had been mistaken, and it is said that he characterised Bathybius as 'not having fulfilled the promise of its youth.' Will any of our present oceanographic beliefs

share the fate of Bathybius in the future? Some may, but even if they do they may well have been useful steps in the progress of science. Although like Bathybius they may not have fulfilled the promise of their youth, yet, we may add, they will not have lived in the minds of man in vain.

Many of the phenomena we encounter in oceanographic investigations are so complex, are or may be affected by so many diverse factors, that it is difficult, if indeed possible, to be sure that we are unravelling them aright and that we see the real causes of what we observe.

Some few things we know approximately—nothing completely. We know that the greatest depths of the ocean, about six miles, are a little greater than the highest mountains on land, and Sir John Murray has calculated that if all the land were washed down into the sea the whole globe would be covered by an ocean averaging about two miles in depth.[4] We know the distribution of temperatures and salinities over a great part of the surface and a good deal of the bottom of the oceans, and some of the more important oceanic currents have been charted and their periodic variations, such as those of the Gulf Stream, are being studied. We know a good deal about the organisms floating or swimming in the surface waters (the epi-plankton), and also those brought up by our dredges and trawls from the bottom in many parts of the world—although every expedition still makes large additions to knowledge. The region that is least known to us, both in its physical conditions and also its inhabitants, is the vast zone of intermediate waters lying between the upper few hundred fathoms and the bottom. That is the region that Alexander Agassiz from his observations with closing tow-nets on the *Blake* Expedition supposed to be destitute of life, or at least, as modified by his later observations on the *Albatross*, to be relatively destitute compared with the surface and the bottom, in opposition to the contention of Murray and other oceanographers that an abundant meso-plankton was present, and that certain groups of animals, such as the Challengerida and some kinds of Medusæ, were characteristic of these deeper zones. I believe that, as sometimes happens in scientific controversies, both sides were right up to a point, and both could support their views upon observations from particular regions of the ocean under certain circumstances.

But much still remains unknown or only imperfectly known even in matters that have long been studied and where practical applications

[4] It was possibly in such a former world-wide ocean of ionised water that according to the recent speculations of A. H. Church (Thalassiophyta, 1919) the first living [illegible] were evolved to become later the floating unicellular plants of th[illegible] plankton.

of great value are obtained—such as the investigation and prediction of tidal phenomena. We are now told that theories require re-investigation and that published tables are not sufficiently accurate. To take another practical application of oceanographic work, the ultimate causes of variations in the abundance, in the sizes, in the movements and in the qualities of the fishes of our coastal industries are still to seek, and notwithstanding volumes of investigation and a still greater volume of discussion, no man who knows anything of the matter is satisfied with our present knowledge of even the best-known and economically most important of our fishes such as the Herring, the Cod, the Plaice and the Salmon.

Take the case of our common fresh-water eel as an example of how little we know and at the same time of how much has been discovered. All the eels of our streams and lakes of N.-W. Europe live and feed and grow under our eyes without reproducing their kind—no spawning eel has ever been seen. After living for years in immaturity, at last near the end of their lives the large male and female yellow eels undergo a change in appearance and in nature. They acquire a silvery colour and their eyes enlarge, and in this bridal attire they commence the long journey which ends in maturity, reproduction and death. From all the fresh waters they migrate in the autumn to the coast, from the inshore seas to the open ocean and still westward and south to the mid-Atlantic and we know not how much further—for the exact locality and manner of spawning has still to be discovered. The youngest known stages of the Leptocephalus, the larval stage of eels, have been found by the Dane, Dr. Johannes Schmidt, to the west of the Azores where the water is over 2000 fathoms in depth. These were about one-third of an inch in length and were probably not long hatched. I cannot now refer to all the able investigators—Grassi, Hjort and others—who have discovered and traced the stages of growth of the Leptocephalus and its metamorphosis into the 'elvers' or young eels which are carried by the North Atlantic drift back to the coasts of Europe and ascend our rivers in spring in countless myriads; but no man has been more indefatigable and successful in the quest than Dr. Schmidt, who in the various expeditions of the Danish Investigation Steamer *Thor* from 1904 onwards found successively younger and younger stages, and who is during the present summer engaged in a traverse of the Atlantic to the West Indies in the hope of finding the missing link in the chain, the actual spawning fresh-water eel in the intermediate waters somewhere above the abysses of the open ocean.[5]

[5] According to Schmidt's results the European fresh-water eel, in order to be able to [illegible], requires a depth of at least 500 fathoms, a salinity of more than 35·2 per mille and a temperature of more than 7° C. in the required depth.

Again, take the case of an interesting oceanographic observation which, if established, may be found to explain the variations in time and amount of important fisheries. Otto Pettersson in 1910 discovered by his observations in the Gullmar Fjord the presence of periodic submarine waves of deeper salter water in the Kattegat and the fjords of the west coast of Sweden, which draw in with them from the Jutland banks vast shoals of the herrings which congregate there in autumn. The deeper layer consists of 'bankwater' of salinity 32 to 34 per thousand, and as this rolls in along the bottom as a series of huge undulations it forces out the overlying fresher water, and so the herrings living in the bankwater outside are sucked into the Kattegat and neighbouring fjords and give rise to important local fisheries. Pettersson connects the crests of the submarine waves with the phases of the moon. Two great waves of salter water which reached up to the surface took place in November 1910, one near the time of full moon and the other about new moon, and the latter was at the time when the shoals of herring appeared inshore and provided a profitable fishery. The coincidence of the oceanic phenomena with the lunar phases is not, however, very exact, and doubts have been expressed as to the connection; but if established, and even if found to be due not to the moon but to prevalent winds or the influence of ocean currents, this would be a case of the migration of fishes depending upon mechanical causes, while in other cases it is known that migrations are due to spawning needs or for the purpose of feeding, as in the case of the cod and the herring in the west and north of Norway and in the Barents Sea.

Then, turning to a very fundamental matter of purely scientific investigation, we do not know with any certainty what causes the great and all-important seasonal variations in the plankton (or floating minute life of the sea) as seen, for example, in our own home seas, where there is a sudden awakening of microscopic plant life, the Diatoms, in early spring when the water is at its coldest. In the course of a few days the upper layers of the sea may become so filled with organisms that a small silk net towed for a few minutes may capture hundreds of millions of individuals. And these myriads of microscopic forms, after persisting for a few weeks, may disappear as suddenly as they came, to be followed by swarms of Copepoda and many other kinds of minute animals, and these again may give place in the autumn to a second maximum of Diatoms or of the closely related Peridiniales. Of course there are theories as to all these more or less periodic changes in the plankton, such as Liebig's 'law of the minimum,' which limits the production of an organism by the amount of that necessity of existence which is present in least quantity, it may be nitrogen or silicon or

phosphorus. According to Raben it is the accumulation of silicic acid in the sea-water that determines the great increase of Diatoms in spring and again in autumn. Some writers have considered these variations in the plankton to be caused largely by changes in temperature supplemented, according to Ostwald, by the resulting changes in the viscosity of the water; but Murray and others are more probably correct in attributing the spring development of phyto-plankton to the increasing power of the sunlight and its value in photosynthesis.

Let us take next the fact—if it be a fact—that the genial warm waters of the tropics support a less abundant plankton than the cold polar seas. The statement has been made and supported by some investigators and disputed by others, both on a certain amount of evidence. This is possibly a case like some other scientific controversies where both sides are partly in the right, or right under certain conditions. At any rate there are marked exceptions to the generalisation. The German Plankton Expedition in 1889 showed in its results that much larger hauls of plankton per unit volume of water were obtained in the temperate North and South Atlantic than in the tropics between, and that the warm Sargasso Sea had a remarkably scanty microflora. Other investigators have since reported more or less similar results. Lohmann found the Mediterranean plankton to be less abundant than that of the Baltic, gatherings brought back from tropical seas are frequently very scanty, and enormous hauls on the other hand have been recorded from Arctic and Antarctic seas. There is no doubt about the large gatherings obtained in northern waters. I have myself in a few minutes' haul of a small horizontal net in the North of Norway collected a mass of the large Copepod *Calanus finmarchicus* sufficient to be cooked and eaten like potted shrimps by half a dozen of the yacht's company, and I have obtained similar large hauls in the cold Labrador current near Newfoundland. On the other hand, Kofoid and Alexander Agassiz have recorded large hauls of plankton in the Humboldt current off the west coast of America, and during the *Challenger* Expedition some of the largest quantities of plankton were found in the equatorial Pacific. Moreover, it is common knowledge that on occasions vast swarms of some planktonic organism may be seen in tropical waters. The yellow alga Trichodesmium, which is said to have given its name to the Red Sea and has been familiarly known as 'sea-sawdust' since the days of Cook's first voyage,[6] may cover the entire surface over considerable areas of the Indian and South Atlantic Oceans; and some pelagic animals such as Salpæ, Medusæ and Ctenophores are also commonly present in abundance in the tropics. Then, again, American

[6] See Journal of Sir Joseph Banks. This and other swarms were also noticed by Darwin during the voyage of the 'Beagle.'

biologists [7] have pointed out that the warm waters of the West Indies and Florida may be noted for the richness of their floating life for periods of years, while at other times the pelagic organisms become rare and the region is almost a desert sea.

It is probable, on the whole, that the distribution and variations of oceanic currents have more than latitude or temperature alone to do with any observed scantiness of tropical plankton. These mighty rivers of the ocean in places teem with animal and plant life, and may sweep abundance of food from one region to another in the open sea.

But even if it be a fact that there is this alleged deficiency in tropical plankton there is by no means agreement as to the cause thereof. Brandt first attributed the poverty of the plankton in the tropics to the destruction of nitrates in the sea as a result of the greater intensity of the metabolism of denitrifying bacteria in the warmer water; and various other writers since then have more or less agreed that the presence of these denitrifying bacteria, by keeping down to a minimum the nitrogen concentration in tropical waters, may account for the relative scarcity of the phyto-plankton, and consequently of the zoo-plankton, that has been observed. But Gran, Nathansohn, Murray, Hjort and others have shown that such bacteria are rare or absent in the open sea, that their action must be negligible, and that Brandt's hypothesis is untenable. It seems clear, moreover, that the plankton does not vary directly with the temperature of the water. Furthermore, Nathansohn has shown the influence of the vertical circulation in the water upon the nourishment of the phyto-plankton—by rising currents bringing up necessary nutrient materials, and especially carbon dioxide from the bottom layers; and also possibly by conveying the products of the drainage of tropical lands to more polar seas so as to maintain the more abundant life in the colder water.

Pütter's view is that the increased metabolism in the warmer water causes all the available food materials to be rapidly used up, and so puts a check to the reproduction of the plankton.

According to van t'Hoff's law in Chemistry, the rate at which a reaction takes place is increased by raising the temperature, and this probably holds good for all bio-chemical phenomena, and therefore for the metabolism of animals and plants in the sea. This has been verified experimentally in some cases by J. Loeb. The contrast between the plankton of Arctic and Antarctic zones, consisting of large numbers of small Crustaceans belonging to comparatively few species, and that of tropical waters, containing a great many more species generally of smaller size and fewer in number of individuals, is to be

[7] A Agassiz, A. G. Mayer, and H. B. Bigelow.

accounted for, according to Sir John Murray and others, by the rate of metabolism in the organisms. The assemblages captured in cold polar waters are of different ages and stages, young and adults of several generations occurring together in profusion,[8] and it is supposed that the adults 'may be ten, twenty or more years of age.' At the low temperature the action of putrefactive bacteria and of enzymes is very slow or in abeyance, and the vital actions of the Crustacea take place more slowly and the individual lives are longer. On the other hand, in the warmer waters of the tropics the action of the bacteria is more rapid, metabolism in general is more active, and the various stages in the life-history are passed through more rapidly, so that the smaller organisms of equatorial seas probably only live for days or weeks in place of years.

This explanation may account also for the much greater quantity of living organisms which has been found so often on the sea floor in polar waters. It is a curious fact that the development of the polar marine animals is in general 'direct' without larval pelagic stages, the result being that the young settle down on the floor of the ocean in the neighbourhood of the parent forms, so that there come to be enormous congregations of the same kind of animal within a limited area, and the dredge will in a particular haul come up filled with hundreds, it may be, of an Echinoderm, a Sponge, a Crustacean, a Brachiopod, or an Ascidian; whereas in warmer seas the young pass through a pelagic stage and so become more widely distributed over the floor of the ocean. The *Challenger* Expedition found in the Antarctic certain Echinoderms, for example, which had young in various stages of development attached to some part of the body of the parents, whereas in temperate or tropical regions the same class of animals set free their eggs and the development proceeds in the open water quite independently of, and it may be far distant from, the parent.

Another characteristic result of the difference in temperature is that the secretion of carbonate of lime in the form of shells and skeletons proceeds more rapidly in warm than in cold water. The massive shells of molluscs, the vast deposits of carbonate of lime formed by corals and by calcareous seaweeds, are characteristic of the tropics; whereas in polar seas, while the animals may be large, they are for the most part soft-bodied and destitute of calcareous secretions. The calcareous pelagic Foraminifera are characteristic of tropical and sub-tropical plankton, and few, if any, are found in polar waters. Globigerina

[8] Whether, however, the low temperature may not also retard reproduction is worthy of consideration.

ooze, a calcareous deposit, is abundant in equatorial seas, while in the Antarctic the characteristic deposit is siliceous Diatomaceous ooze.

The part played by bacteria in the metabolism of the sea is very important and probably of wide-reaching effect, but we still know very little about it. A most promising young Cambridge biologist, the late Mr. G. Harold Drew, now unfortunately lost to science, had already done notable work at Jamaica and at Tortugas, Florida, on the effects produced by a bacillus which is found in the surface waters of these shallow tropical seas and in the mud at the bottom; and which denitrifies nitrates and nitrites, giving off free nitrogen. He found that this *Bacillus calcis* also caused the precipitation of soluble calcium salts in the form of calcium carbonate (' drewite ') on a large scale, in the warm shallow waters. Drew's observations tend to show that the great calcareous deposits of Florida and the Bahamas previously known as ' coral muds ' are not, as was supposed by Murray and others, derived from broken-up corals, shells, nullipores, &c., but are minute particles of carbonate of lime which have been precipitated by the action of these bacteria.[9]

The bearing of these observations upon the formation of oolitic limestones and the fine-grained unfossiliferous Lower Palæozoic limestones of New York State, recently studied in this connection by R. M. Field,[10] must be of peculiar interest to geologists, and forms a notable instance of the annectant character of Oceanography, bringing the metabolism of living organisms in the modern sea into relation with palæozoic rocks.

The work of marine biologists on the plankton has been in the main *qualitative*, the identification of species, the observation of structure, and the tracing of life-histories. The oceanographer adds to that the *quantitative* aspect when he attempts to estimate numbers and masses per unit volume of water or of area. Let me lay before you a few thoughts in regard to some such attempts, mainly for the purpose of showing the difficulties of the investigation. Modern quantitative methods owe their origin to the ingenious and laborious work of Victor Hensen, followed by Brandt, Apstein, Lohmann, and others of the Kiel school of quantitative planktologists. We may take their well-known estimations of fish eggs in the North Sea as an example of the method.

The floating eggs and embryos of our more important food fishes may occur in quantities in the plankton during certain months in spring, and Hensen and Apstein have made some notable calculations

[9] *Journn. Mar. Biol. Assoc.*, October 1911

[10] Carnegie Institute of Washington. Year Book for 1919, p. 197.

based on the occurrence of these in certain hauls taken at intervals across the North Sea, which led them to the conclusion that, taking six of our most abundant fish, such as the cod and some of the flat fish, the eggs present were probably produced by about 1200 million spawners, enabling them to calculate that the total fish population of the North Sea (of these six species), at that time (spring of 1895), amounted to about 10,000 millions. Further calculations led them to the result that the fishermen's catch of these fishes amounted to about one-quarter of the total population. Now all this is not only of scientific interest, but also of great practical importance if we could be sure that the samples upon which the calculations are based were adequate and representative, but it will be noted that these samples only represent one square metre in 3,465,968,354. Hensen's statement, repeated in various works in slightly differing words, is to the effect that, using a net of which the constants are known hauled vertically through a column of water from a certain depth to the surface, he can calculate the volume of water filtered by the net and so estimate the quantity of plankton under each square metre of the surface; and his whole results depend upon the assumption, which he considers justified, that the plankton is evenly distributed over large areas of water which are under similar conditions. In these calculations in regard to the fish eggs he takes the whole of the North Sea as being an area under similar conditions, but we have known since the days of P. T. Cleve and from the observations of Hensen's own colleagues that this is not the case, and they have published chart-diagrams showing that at least three different kinds of water under different conditions are found in the North Sea, and that at least five different planktonic areas may be encountered in making a traverse from Germany to the British Isles. If the argument be used that wherever the plankton is found to vary there the conditions cannot be uniform, then few areas of the ocean of any considerable size remain as cases suitable for population-computation from random samples. It may be doubted whether even the Sargasso Sea, which is an area of more than usually uniform character, has a sufficiently evenly distributed plankton to be treated by Hensen's method of estimation of the population.

In the German Plankton Expedition of 1889 Schütt reports that in the Sargasso Sea, with its relatively high temperature, the twenty-four catches obtained were uniformly small in quantity. His analysis of the volumes of these catches shows that the average was 3·33 c.c., but the individual catches ranged from 1·5 c.c. to 6·5 c.c., and the divergence from the average may be as great as + 3·2 c.c.; and, after deducting 20 per cent. of the divergence as due to errors of the experiment,

Schütt estimates the mean variation of the plankton at about 16 per cent. above or below. This does not seem to me to indicate the uniformity that might be expected in this 'halistatic' area occupying the centre of the North Atlantic Gulf Stream circulation. Hensen also made almost simultaneous hauls with the same net in quick succession to test the amount of variation, and found that the average error was about 13 per cent.

As so much depends in all work at sea upon the weather, the conditions under which the ship is working, and the care taken in the experiment, with the view of getting further evidence under known conditions I carried out some similar experiments at Port Erin on four occasions during last April and on a further occasion a month later, choosing favourable weather and conditions of tide and wind, so as to be able to maintain an approximate position. On each of four days in April the Nansen net, with No. 20 silk, was hauled six times from the same depth (on two occasions 8 fathoms and on two occasions 20 fathoms), the hauls being taken in rapid succession and the catches being emptied from the net into bottles of 5 per cent. formaline, in which they remained until examined microscopically.

The results were of interest, for although they showed considerable uniformity in the amount of the catch—for example, six successive hauls from 8 fathoms being all of them 0·2 c.c. and four out of five from 20 fathoms being 0·6 c.c.—the volume was made up rather differently in the successive hauls. The same organisms are present for the most part in each haul, and the chief groups of organisms are present in much the same proportion. For example, in a series where the Copepoda average about 100 the Dinoflagellates average about 300 and the Diatoms about 8000, but the percentage deviation of individual hauls from the average may be as much as *plus* or *minus* 50. The numbers for each organism (about 40) in each of the twenty-six hauls have been worked out, and the details will be published elsewhere, but the conclusion I come to is that if on each occasion one haul only, in place of six, had been taken, and if one had used that haul to estimate the abundance of any one organism in that sea-area, one might have been about 50 per cent. wrong in either direction.

Successive improvements and additions to Hensen's methods in collecting plankton have been made by Lohmann, Apstein, Gran, and others, such as pumping up water of different layers through a hose-pipe and filtering it through felt, filter-paper, and other materials which retain much of the micro-plankton that escapes through the meshes of the finest silk. Use has even been made of the extraordinarily minute and beautifully regular natural filter spun by the pelagic animal *Appendicularia* for the capture of its own food. This grid-like trap,

when dissected out and examined under the microscope, reveals a surprising assemblage of the smallest protozoa and protophyta, less than 30 micro-millimetres in diameter, which would all pass easily through the meshes of our finest silk nets.

The latest refinement in capturing the minutest-known organisms of the plankton (excepting the bacteria) is a culture method devised by Dr. E. J. Allen, Director of the Plymouth Laboratory.[11] By diluting half a cubic centimetre of the sea-water with a considerable amount (1500 c.c.) of sterilised water treated with a nutrient solution, and distributing that over a large number (70) of small flasks in which after an interval of some days the number of different kinds of organisms which had developed in each flask were counted, he calculates that the sea contains 464,000 of such organisms per litre; and he gives reasons why his cultivations must be regarded as minimum results, and states that the total per litre may well be something like a million. Thus every new method devised seems to multiply many times the probable total population of the sea. As further results of the quantitative method it may be recorded that Brandt found about 200 diatoms per drop of water in Kiel Bay, and Hensen estimated that there are several hundred millions of diatoms under each square metre of the North Sea or the Baltic. It has been calculated that there is approximately one Copepod in each cubic inch of Baltic water, and that the annual consumption of these Copepoda by herring is about a thousand billion; and that in the 16 square miles of a certain Baltic fishery there is Copepod food for over 530 millions of herring of an average weight of 60 grammes.

There are many other problems of the plankton in addition to quantitative estimates—probably some that we have not yet recognised—and various interesting conclusions may be drawn from recent planktonic observations. Here is a case of the introduction and rapid spread of a form new to British seas.

Biddulphia sinensis is an exotic diatom which, [illegible] to Ostenfeld, made its appearance at the mouth of the Elbe in 1903, and spread during successive years in several directions. It appeared suddenly in our plankton gatherings at Port Erin in November 1909, and has been present in abundance each year since. Ostenfeld, in 1908, when tracing its spread in the North Sea, found that the migration to the north along the coast of Denmark to Norway corresponded with the rate of flow of the Jutland current to the Skager Rak—viz., about 17 cm. per second—a case of plankton distribution throwing light on hydrography—and he predicted that it would soon be found in the English

[11] *Journ. Mar. Biol. Assoc.* xii. 1, July 1919.

Channel. Dr. Marie Lebour, who recently examined the store of plankton gatherings at the Plymouth Laboratory, finds that as a matter of fact this form did appear in abundance in the collections of October 1909, within a month of the time when according to our records it reached Port Erin. Whether or not this is an Indo-Pacific species brought accidentally by a ship from the Far East, or whether it is possibly a new mutation which appeared suddenly in our seas, there is no doubt that it was not present in our Irish Sea plankton gatherings previous to 1909, but has been abundant since that year, and has completely adopted the habits of its English relations—appearing with *B. mobiliensis* in late autumn, persisting during the winter, reaching a maximum in spring, and dying out before summer.

The Nauplius and Cypris stages of *Balanus* in the plankton form an interesting study. The adult barnacles are present in enormous abundance on the rocks round the coast, and they reproduce in winter, at the beginning of the year. The newly emitted young are sometimes so abundant as to make the water in the shore pools and in the sea close to shore appear muddy. The Nauplii first appeared at Port Erin, in 1907, in the bay gatherings on February 22 (in 1908 on February 13), and increased with ups and downs to their maximum on April 15, and then decreased until their disappearance on April 26. None were taken at any other time of the year. The Cypris stage follows on after the Nauplius. It was first taken in the bay on April 6, rose to its maximum on the same day with the Nauplii, and was last caught on May 24. Throughout, the Cypris curve keeps below that of the Nauplius, the maxima being 1740 and 10,500 respectively. Probably the difference between the two curves represents the death-rate of *Balanus* during the Nauplius stage. That conclusion I think we are justified in drawing, but I would not venture to use the result of any haul, or the average of a number of hauls, to multiply by the number of square yards in a zone round our coast in order to obtain an estimate of the number of young barnacles, or of the old barnacles that produced them—the irregularities are too great.

To my mind it seems clear that there must be three factors making for irregularity in the distribution of a plankton organism:—

1. The sequence of stages in its life-history—such as the Nauplius and Cypris stages of *Balanus*.

2. The results of interaction with other organisms—as when a swarm of *Calanus* is pursued and devoured by a shoal of herring.

3. Abnormalities in time or abundance due to the physical environment—as in favourable or unfavourable seasons.

And these factors must be at work in the open ocean as well as in coastal waters.

In many oceanographical inquiries there is a double object. There is the scientific interest and there is the practical utility—the interest, for example, of tracing a particular swarm of a Copepod like *Calanus*, and of making out why it is where it is at a particular time, tracing it back to its place of origin, finding that it has come with a particular body of water, and perhaps that it is feeding upon a particular assemblage of Diatoms; endeavouring to give a scientific explanation of every stage in its progress. Then there is the utility—the demonstration that the migration of the *Calanus* has determined the presence of a shoal of herrings or mackerel that are feeding upon it, and so have been brought within the range of the fisherman and have constituted a commercial fishery.

We have evidence that pelagic fish which congregate in shoals, such as herring and mackerel, feed upon the Crustacea of the plankton and especially upon Copepoda. A few years ago when the summer herring fishery off the south end of the Isle of Man was unusually near the land, the fishermen found large red patches in the sea where the fish were specially abundant. Some of the red stuff, brought ashore by the men, was examined at the Port Erin Laboratory and found to be swarms of the Copepod *Temora longicornis*; and the stomachs of the herring caught at the same time were engorged with the same organism. It is not possible to doubt that during these weeks of the herring fishery in the Irish Sea the fish were feeding mainly upon this species of Copepod. Some ten years ago Dr. E. J. Allen and Mr. G. E. Bullen published [12] some interesting work, from the Plymouth Marine Laboratory, demonstrating the connection between mackerel and Copepoda and sunshine in the English Channel; and Farran [13] states that in the spring fishery on the West of Ireland the food of the mackerel is mainly composed of *Calanus*.

Then again at the height of the summer mackerel fishery in the Hebrides, in 1913, we found [14] the fish feeding upon the large Copepod *Calanus finmarchicus*, which was caught in the tow-net at the rate of about 6000 in a five-minutes' haul, and 6000 was also the average number found in the stomachs of the fish caught at the same time.

These were cases where the fish were feeding upon the organism that was present in swarms—a monotonic plankton—but in other cases the fish are clearly selective in their diet. If the sardine of the French coast can pick out from the micro-plankton the minute Peridiniales in preference to the equally minute Diatoms which are present in the sea at the same time, there seems no reason why the herring and the

[12] *Journ. Mar. Biol. Assoc.* vol. viii. (1909), pp. 394-406.
[13] *Conseil Internat. Bull. Trimestr.* 1902-8, 'Planktonique,' p. 89.
[14] 'Spolia Runiana,' iii. *Linn. Soc. Journ., Zoology*, vol. xxxiv p. 95, 1918.

mackerel should not be able to select particular species of Copepoda or other large organisms from the macro-plankton, and we have evidence that they do. Nearly thirty years ago the late Mr. Isaac Thompson, a constant supporter of the Zoological Section of this Association and one of the Honorary Local Secretaries for the last Liverpool meeting, showed me in 1893 that young plaice at Port Erin were selecting one particular Copepod, a species of Jonesiella, out of many others caught in our tow-nets at the time. H. Blegvad[15] showed in 1916 that young food fishes and also small shore fishes pick out certain species of Copepoda (such as Harpacticoids) and catch them individually —either lying in wait or searching for them. A couple of years later[16] Dr. Marie Lebour published a detailed account of her work at Plymouth on the food of young fishes, proving that certain fish undoubtedly do prefer certain planktonic food.

These Crustacea of the plankton feed upon smaller and simpler organisms—the Diatoms, the Peridinians, and the Flagellates—and the fish themselves in their youngest post-larval stages are nourished by the same minute forms of the plankton. Thus it appears that our sea-fisheries ultimately depend upon the living plankton which no doubt in its turn is affected by hydrographic conditions. A correlation seems to be established between the Cornish pilchard fisheries and periodic variations in the physical characters (probably the salinity) of the water of the English Channel between Plymouth and Jersey.[17] Apparently a diminished intensity in the Atlantic current corresponds with a diminished fishery in the following summer. Possibly the connection in these cases is through an organism of the plankton.

It is only a comparatively small number of different kinds of organisms—both plants and animals—that make up the bulk of the plankton that is of real importance to fish. One can select about half-a-dozen species of Copepoda which constitute the greater part of the summer zoo-plankton suitable as food for larval or adult fishes, and about the same number of generic types of Diatoms which similarly make up the bulk of the available spring phyto-plankton year after year. This fact gives great economic importance to the attempt to determine with as much precision as possible the times and conditions of occurrence of these dominant factors of the plankton in an average year. An obvious further extension of this investigation is an inquiry into the degree of coincidence between the times of appearance in the sea of the plankton organisms and of the young fish, and the possible effect of any marked absence of correlation in time and quantity.

Just before the war the International Council for the Exploration

[15] *Rep. Danish Biol. Stat.* xxiv. 1916.
[16] *Journ. Mar. Biol. Assoc.* May 1918.
[17] See E. C. Jee, *Hydrography of the English Channel*, 1904-17.

of the Sea[18] arrived at the conclusion that fishery investigations indicated the probability that the great periodic fluctuations in the fisheries are connected with the fish larvæ being developed in great quantities only in certain years. Consequently they advised that plankton work should be directed primarily to the question whether these fluctuations depend upon differences in the plankton production in different years. It was then proposed to begin systematic investigation of the fish larvæ and the plankton in spring and to determine more definitely the food of the larval fish at various stages.

About the same time Dr. Hjort[19] made the interesting suggestion that possibly the great fluctuations in the number of young fish observed from year to year may not depend wholly upon the number of eggs produced, but also upon the relation in time between the hatching of these eggs and the appearance in the water of the enormous quantity of Diatoms and other plant plankton upon which the larval fish after the absorption of their yolk depend for food. He points out that, if even a brief interval occurs between the time when the larvæ first require extraneous nourishment and the period when such food is available, it is highly probable that an enormous mortality would result. In that case even a rich spawning season might yield but a poor result in fish in the commercial fisheries of successive years for some time to come. So that, in fact, the numbers of a year-class may depend not so much upon a favourable spawning season as upon a coincidence between the hatching of the larvæ and the presence of abundance of phyto-plankton available as food.[20]

The curve for the spring maximum of Diatoms corresponds in a general way with the curve representing the occurrence of pelagic fish eggs in our seas. But is the correspondence sufficiently exact and constant to meet the needs of the case? The phyto-plankton may still be relatively small in amount during February and part of March in some years, and it is not easy to determine exactly when, in the open sea, the fish eggs have hatched out in quantity and the larvæ have absorbed their food-yolk and started feeding on Diatoms.

If, however, we take the case of one important fish—the plaice—we can get some data from our hatching experiments at the Port Erin Biological Station which have now been carried on for a period of seventeen years. An examination of the hatchery records for these years in comparison with the plankton records of the neighbouring sea, which have been kept systematically for the fourteen years from 1907

[18] *Rapports et Proc. Verb.* xix. December 1913.

[19] *Rapports et Proc. Verb.* xx. 1914, p. 204.

[20] For the purpose of this argument we may include in 'phyto-plankton' the various [illegible] Flagellata and other minute organisms [illegible] present with [illegible].

to 1920 inclusive, shows that in most of these years the Diatoms were present in abundance in the sea a few days at least before the fish larvæ from the hatchery were set free, and that it was only in four years (1908, '09, '13, and '14) that there was apparently some risk of the larvæ finding no phyto-plankton food, or very little. The evidence so far seems to show that if fish larvæ are set free in the sea as late as March 20, they are fairly sure of finding suitable food;[21] but if they are hatched as early as February they run some chance of being starved.

But this does not exhaust the risks to the future fishery. C. G. Joh. Petersen and Boysen-Jensen in their valuation of the Limfjord[22] have shown that in the case not only of some fish but also of the larger invertebrates on which they feed there are marked fluctuations in the number of young produced in different seasons, and that it is only at intervals of years that a really large stock of young is added to the population.

The prospects of a year's fishery may therefore depend primarily upon the rate of spawning of the fish, affected no doubt by hydrographic and other environmental conditions, secondarily upon the presence of a sufficient supply of phyto-plankton in the surface layers of the sea at the time when the fish larvæ are hatched, and that in its turn depends upon photosynthesis and physico-chemical changes in the water, and finally upon the reproduction of the stock of molluscs or worms at the bottom which constitute the fish food at later stages of growth and development.

The question has been raised of recent years—Is there enough plankton in the sea to provide sufficient nourishment for the larger animals, and especially for those fixed forms such as sponges that are supposed to feed by drawing currents of plankton-laden water through the body? In a series of remarkable papers from 1907 onwards Pütter and his followers put forward the views (1) that the carbon requirements of such animals could not be met by the amount of plankton in the volume of water that could be passed through the body in a given time, and (2) that sea-water contained a large amount of dissolved organic carbon compounds which constitute the chief if not the only food of a large number of marine animals. These views have given rise to much controversy and have been useful in stimulating further research, but I believe it is now admitted that Pütter's samples of water from the Bay of Naples and at Kiel were probably polluted, that his figures were erroneous, and that his conclusions

[21] All dates and statements as to occurrence refer to the Irish Sea round the south end of the Isle of Man. For further details see *Report Lancs. Sea-Fish. Lab.* for 1919.

[22] *Report of Danish Biol. Station for* 1919.

must be rejected, or at least greatly modified. His estimates of the plankton were minimum ones, while it seems probable that his figures for the organic carbon present represent a variable amount of organic matter arising from one of the reagents used in the analyses.[23] The later experimental work of Henze, of Raben, and of Moore shows that the organic carbon dissolved in sea-water is an exceedingly minute quantity, well within the limits of experimental error. Moore puts it, at the most, at one-millionth part, or 1 mgm. in a litre. At the Dundee meeting of the Association in 1912 a discussion on this subject took place, at which Pütter still adhered to a modified form of his hypothesis of the inadequacy of the plankton and the nutrition of lower marine animals by the direct absorption of dissolved organic matter. Further work at Port Erin since has shown that, while the plankton supply as found generally distributed would prove sufficient for the nutrition of such sedentary animals as Sponges and Ascidians, which require to filter only about fifteen times their own volume of water per hour, it is quite inadequate for active animals such as Crustaceans and Fishes. These latter are, however, able to seek out and capture their food, and are not dependent on what they may filter or absorb from the sea-water. This result accords well with recorded observations on the irregularity in the distribution of the plankton, and with the variations in the occurrence of the migratory fishes which may be regarded as following and feeding upon the swarms of planktonic organisms.

This then, like most of the subjects I am dealing with, is still a matter of controversy, still not completely understood. Our need, then, is Research, more Research, and *still more Research.*

Our knowledge of the relations between plankton productivity and variation and the physico-chemical environment is still in its infancy, but gives promise of great results in the hands of the bio-chemist and the physical chemist.

Recent papers by Sørensen, Palitzsch, Witting, Moore, and others have made clear that the amount of hydrogen-ion concentration as indicated by the relative degree of alkalinity and acidity in the sea-water may undergo local and periodic variations and that these have an effect upon the living organisms in the water and can be correlated with their presence and abundance. To take an example from our own seas, Professor Benjamin Moore and his assistants in their work at the Port Erin Biological Station in successive years from 1912 onwards have shown [24] that the sea around the Isle of Man is a good deal more alkaline in spring (say April) than it is in summer (say

[23] See Moore, etc., *Bio-Chem. Journ.* vi. p. 266, 1912.

[24] 'Photosynthetic phenomena in sea-water,' *Trans. Liverpool Biol. Soc.* xxix. 233, 1915.

July). The alkalinity, which gets low in summer, increases somewhat in autumn, and then decreases rapidly, to disappear during the winter; and then once more, after several months of a minimum, begins to come into evidence again in March, and rapidly rises to its maximum in April or May. This periodic change in alkalinity will be seen to correspond roughly with the changes in the living microscopic contents of the sea represented by the phyto-plankton annual curve, and the connection between the two will be seen when we realise that the alkalinity of the sea is due to the relative absence of carbon dioxide. In early spring, then, the developing myriads of diatoms in their metabolic processes gradually use up the store of carbon dioxide accumulated during the winter, or derived from the bi-carbonates of calcium and magnesium, and so increase the alkalinity of the water, till the maximum of alkalinity, due to the fixation of the carbon and the reduction in amount of carbon dioxide, corresponds with the crest of the phyto-plankton curve in, say, April. Moore has calculated that the annual turnover in the form of carbon which is used up or converted from the inorganic into an organic form probably amounts to something of the order of 20,000 or 30,000 tons of carbon per cubic mile of sea-water, or, say, over an area of the Irish Sea measuring 16 square miles and a depth of 50 fathoms; and this probably means a production each season of about two tons of dry organic matter, corresponding to at least ten tons of moist vegetation, per acre—which suggests that we may still be very far from getting from our seas anything like the amount of possible food-matters that are produced annually.

Testing the alkalinity of the sea-water may therefore be said to be merely ascertaining and measuring the results of the photosynthetic activity of the great phyto-plankton rise in spring due to the daily increase of sunlight.

The marine biologists of the Carnegie Institute, Washington, have made a recent contribution to the subject in certain observations on the alkalinity of the sea (as determined by hydrogen-ion concentration), during which they found in tropical mid-Pacific a sudden change to acidity in a current running eastwards. Now in the Atlantic the Gulf Stream, and tropical Atlantic waters generally, are much more alkaline than the colder coastal water running south from the Gulf of St. Lawrence. That is, the colder Arctic water has more carbon dioxide. This suggests that the Pacific easterly set may be due to deeper water, containing more carbon dioxide (=acidity), coming to the surface at that point. The alkalinity of the sea-water can be determined rapidly by mixing the sample with a few drops of an indicator and observing the change of colour; and this method of detecting ocean currents by observing the hydrogen-ion concentration of the water might be useful to navigators as showing the time of entrance to a known current.

Oceanography has many practical applications, chiefly, but by no means wholly, on the biological side. The great fishing industries of the world deal with living organisms, of which all the vital activities and the inter-relations with the environment are matters of scientific investigation. Aquiculture is as susceptible of scientific treatment as agriculture can be; and the fisherman who has been in the past too much the nomad and the hunter—if not, indeed, the devastating raider—must become in the future the settled farmer of the sea if his harvest is to be less precarious. Perhaps the nearest approach to cultivation of a marine product, and of the fisherman reaping what he has actually sown, is seen in the case of the oyster and mussel industries on the west coast of France, in Holland, America, and to a less extent on our own coast. Much has been done by scientific men for these and other similar coastal fisheries since the days when Professor Coste in France in 1859 introduced oysters from the Scottish oyster-beds to start the great industry at Arcachon and elsewhere. Now we buy back the descendants of our own oysters from the French ostreiculturists to replenish our depleted beds.

It is no small matter to have introduced a new and important food-fish to the markets of the world. The remarkable deep-water "tile-fish," new to science and described as *Lopholatilus chamæleonticeps*, was discovered in 1879 by one of the United States fishing schooners to the south of Nantucket, near the 100-fathom line. Several thousand pounds weight were caught, and the matter was duly investigated by the United States Fish Commission. For a couple of years after that the fish was brought to market in quantity, and then something unusual happened at the bottom of the sea, and in 1882 millions of dead tile-fish were found floating on the surface over an area of thousands of square miles. The schooner *Navarino* sailed for two days and a night through at least 150 miles of sea, thickly covered as far as the eye could reach with dead fish, estimated at 256,000 to the square mile. The Fish Commission sent a vessel to fish systematically over the grounds known as the 'Gulf Stream slope,' where the tile-fish had been so abundant during the two previous years, but she did not catch a single fish, and the associated sub-tropical invertebrate fauna was also practically obliterated.

This wholesale destruction was attributed by the American oceanographers to a sudden change in the temperature of the water at the bottom, due in all probability to a withdrawal southwards of the warm Gulf Stream water and a flooding of the area by the cold Labrador current.

I am indebted to Dr. C. H. Townsend, Director of the celebrated New York Aquarium, for the latest information in regard to the

reappearance in quantity of this valuable fish upon the old fishing grounds off Nantucket and Long Island, at about 100 miles from the coast to the east and south-east of New York. It is believed that the tile-fish is now abundant enough to maintain an important fishery, which will add an excellent food-fish to the markets of the United States. It is easily caught with lines at all seasons of the year, and reaches a length of over three feet and a weight of 40 to 50 pounds. During July 1915 the product of the fishery was about two and a half million pounds weight, valued at 55,000 dollars, and in the first few months of 1917 the catch was four and a half million pounds, for which the fishermen received 247,000 dollars.

We can scarcely hope in European seas to add new food-fishes to our markets, but much may be done through the co-operation of scientific investigators of the ocean with the Administrative Departments to bring about a more rational conservation and exploitation of the national fisheries.

Earlier in this address I referred to the pioneer work of the distinguished Manx naturalist, Professor Edward Forbes. There are many of his writings and of his lectures which I have no space to refer to which have points of oceanographic interest. Take this, for example, in reference to our national sea fisheries. We find him in 1847 writing to a friend: 'On Friday night I lectured at the Royal Institution. The subject was the bearing of submarine researches and distribution matters on the fishery question. I pitched into Government mismanagement pretty strong, and made a fair case of it. It seems to me that at a time when half the country is starving we are utterly neglecting or grossly mismanaging great sources of wealth and food. . . . Were I a rich man I would make the subject a hobby, for the good of the country and for the better proving that the true interests of Government are those linked with and inseparable from Science.' We must still cordially approve of these last words, while recognising that our Government Department of Fisheries is now being organised on better lines, is itself carrying on scientific work of national importance, and is, I am happy to think, in complete sympathy with the work of independent scientific investigators of the sea and desirous of closer co-operation with University laboratories and biological stations.

During recent years one of the most important and most frequently discussed of applications of fisheries investigation has been the productivity of the trawling grounds, and especially those of the North Sea. It has been generally agreed that the enormous increase of fishing power during the last forty years or so has reduced the number of large plaice, so that the average size of that fish caught in our home

waters has become smaller, although the total number of plaice landed had continued to increase up to the year of the outbreak of war. Since then, from 1914 to 1919, there has of necessity been what may be described as the most gigantic experiment ever seen in the closing of extensive fishing grounds. It is still too early to say with any certainty exactly what the results of that experiment have been, although some indications of an increase of the fish population in certain areas have been recorded. For example, the Danes, A. C. Johansen and Kirstine Smith, find that large plaice landed in Denmark are now more abundant, and they attribute this to a reversal of the pre-war tendency, due to less intensive fishing. But Dr. James Johnstone has pointed out that there is some evidence of a natural periodicity in abundance of such fish and that the results noticed may represent phases in a cyclic change. If the periodicity noted in Liverpool Bay [25] holds good for other grounds it will be necessary in any comparison of pre-war and post-war statistics to take this natural variation in abundance into very careful consideration.

In the application of oceanographic investigations to sea-fisheries problems, one ultimate aim, whether frankly admitted or not, must be to obtain some kind of a rough approximation to a census or valuation of the sea—of the fishes that form the food of man, of the lower animals of the sea-bottom on which many of the fishes feed, and of the planktonic contents of the upper waters which form the ultimate organised food of the sea—and many attempts have been made in different ways to attain the desired end.

Our knowledge of the number of animals living in different regions of the sea is for the most part relative only. We know that one haul of the dredge is larger than another, or that one locality seems richer than another, but we have very little information as to the actual numbers of any kind of animal per square foot or per acre in the sea. Hensen, as we have seen, attempted to estimate the number of food-fishes in the North Sea from the number of their eggs caught in a comparatively small series of hauls of the tow-net, but the data were probably quite insufficient and the conclusions may be erroneous. It is an interesting speculation to which we cannot attach any economic importance. Heincke says of it: 'This method appears theoretically feasible, but presents in practice so many serious difficulties that no positive results of real value have as yet been obtained.'

All biologists must agree that to determine even approximately the number of individuals of any particular species living in a known area is a contribution to knowledge which may be of great economic value

[25] See Johnstone, *Report Lancs. Sea-Fish. Lab.* for 1917, p. 60; and Daniel, *Report* for 1919, p. 51.

in the case of the edible fishes, but it may be doubted whether Hensen's methods, even with greatly increased data, will ever give us the required information. Petersen's method, of setting free marked plaice and then assuming that the proportion of these recaught is to the total number marked as the fishermen's catch in the same district is to the total population, will only hold good in circumscribed areas where there is practically no migration and where the fish are fairly evenly distributed. This method gives us what has been called 'the fishing coefficient,' and this has been estimated for the North Sea to have a probable value of about 0·33 for those sizes of fish which are caught by the trawl. Heincke,[26] from an actual examination of samples of the stock on the ground obtained by experimental trawling ('the catch coefficient'), supplemented by the market returns of the various countries, estimates the adult plaice at about 1,500 millions, of which about 500 millions are caught or destroyed by the fishermen annually.

It is difficult to imagine any further method which will enable us to estimate any such case as, say, the number of plaice in the North Sea where the individuals are so far beyond our direct observation and are liable to change their positions at any moment. But a beginning can be made on more accessible ground with more sedentary animals, and Dr. C. G. Joh. Petersen, of the Danish Biological Station, has for some years been pursuing the subject in a series of interesting Reports on the 'Evaluation of the Sea.'[27] He uses a bottom-sampler, or grab, which can be lowered down open and then closed on the bottom so as to bring up a sample square foot or square metre (or in deep water one-tenth of a square metre) of the sand or mud and its inhabitants. With this apparatus, modified in size and weight for different depths and bottoms, Petersen and his fellow-workers have made a very thorough examination of the Danish waters, and especially of the Kattegat and the Limfjord, have described a series of 'animal communities' characteristic of different zones and regions of shallow water, and have arrived at certain numerical results as to the quantity of animals in the Kattegat expressed in tons—such as 5,000 tons of plaice requiring as food 50,000 tons of 'useful animals' (mollusca and polychaet worms), and 25,000 tons of starfish using up 200,000 tons of useful animals which might otherwise serve as food for fishes, and the dependence of all these animals directly or indirectly upon the great Beds of Zostera, which make up 24,000,000 tons in Kattegat. Such estimates are obviously of great biological interest, and even if only rough approximations are a valuable contribution to our under-

[26] F. Heincke, *Cons. Per. Internat. Explor. de la Mer*, 'Investigations on the Plaice,' Copenhagen, 1913.

[27] See *Reports of the Danish Biological Station*, and especially the *Report* for 1918 'The Sea Bottom and its Production of Fish Food.'

standing of the metabolism of the sea and of the possibility of increasing the yield of local fisheries.

But on studying these Danish results in the light of what we know of our own marine fauna, although none of our seas have been examined in the same detail by the bottom-sampler method, it seems probable that the animal communities as defined by Petersen are not exactly applicable on our coasts and that the estimates of relative and absolute abundance may be very different in different seas under different conditions. The work will have to be done in each great area, such as the North Sea, the English Channel, and the Irish Sea, independently. This is a necessary investigation, both biological and physical, which lies before the oceanographers of the future, upon the results of which the future preservation and further cultivation of our national sea-fisheries may depend.

It has been shown by Johnstone and others that the common edible animals of the shore may exist in such abundance that an area of the sea may be more productive of food for man than a similar area of pasture or crops on land. A Lancashire mussel bed has been shown to have as many as 16,000 young mussels per square foot, and it is estimated that in the shallow waters of Liverpool Bay there are from twenty to 200 animals of sizes varying from an amphipod to a plaice on each square metre of the bottom.[28]

From these and similar data which can be readily obtained, it is not difficult to calculate totals by estimating the number of square yards in areas of similar character between tide-marks or in shallow water. And from weighings of samples some approximation to the number of tons of available food may be computed. But one must not go too far. Let all the figures be based upon actual observation. Imagination is necessary in science, but in calculating a population of even a very limited area it is best to believe only what one can see and measure.

Countings and weighings, however, do not give us all the information we need. It is something to know even approximately the number of millions of animals on a mile of shore and the number of millions of tons of possible food in a sea-area, but that is not sufficient. All food-fishes are not equally nourishing to man, and all plankton and bottom invertebrata are not equally nourishing to a fish. At this point the biologist requires the assistance of the physiologist and the bio-chemist. We want to know next the value of our food matters in proteids, carbohydrates, and fats, and the resulting calories. Dr. Johnstone, of the Oceanography Department of the University of Liverpool, has already shown us how markedly a fat summer herring

[28] *Conditions of Life in the Sea*, Cambridge Univ. Press, 1908.

differs in essential constitution from the ordinary white fish, such as the cod, which is almost destitute of fat.

Professor Brandt, at Kiel, Professor Benjamin Moore, at Port Erin, and others have similarly shown that plankton gatherings may vary greatly in their nutrient value according as they are composed mainly of Diatoms, of Dinoflagellates, or of Copepoda. And, no doubt, the animals of the 'benthos,' the common invertebrates of our shores, will show similar differences in analysis.[29] It is obvious that some contain more solid flesh, others more water in their tissues, others more calcareous matter in the exoskeleton, and that therefore weight for weight we may be sure that some are more nutritious than the others; and this is probably at least one cause of that preference we see in some of our [illegible] fish for certain kinds of food, such as polychaet worms, in which there is relatively little waste, and thin-shelled lamellibranch molluscs, such as young mussels, which have a highly nutrient body in a comparatively thin and brittle shell.

My object in referring to these still incomplete investigations is to direct attention to what seems a natural and useful extension of faunistic work, for the purpose of obtaining some approximation to a quantitative estimate of the more important animals of our shores and shallow water and their relative values as either the immediate or the ultimate food of marketable fishes.

Each such fish has its 'food-chain' or series of alternative chains, leading back from the food of man to the invertebrates upon which it preys and then to the food of these, and so down to the smallest and simplest organisms in the sea, and each such chain must have all its links fully worked out as to seasonal and quantitative occurrence back to the Diatoms and Flagellates which depend upon physical conditions and take us beyond the range of biology—but not beyond that of oceanography. The Diatoms and the Flagellates are probably more important than the more obvious sea-weeds not only as food, but also in supplying to the water the oxygen necessary for the respiration of living protoplasm. Our object must be to estimate the rate of production and rate of destruction of all organic substances in the sea.

To attain to an approximate census and valuation of the sea—remote though it may seem—is a great aim, but it is not sufficient. We want not only to observe and to count natural objects, but also to understand them. We require to know not merely what an organism is—in the fullest detail of structure and development and affinities—

[29] Moore and others have made analyses [illegible] fat, etc., in the soft parts of Sponge, Asci[illegible] Fusus, [illegible] Cancer at Port Erin, and find considerable [illegible] protein ranging, [illegible] from 8 to 51 per cent., and the fat from 2 to 14 per cent. (see [illegible] *Journ.* vi. p. 291).

and also where it occurs—again in full detail—and in what abundance under different circumstances, but also *how* it lives and what all its relations are to both its physical and its biological environment, and that is where the physiologist, and especially the bio-chemist, can help us. In the best interests of biological progress the day of the naturalist who merely collects, the day of the anatomist and histologist who merely describe, is over, and the future is with the observer and the experimenter animated by a divine curiosity to enter into the life of the organism and understand how it lives and moves and has its being. 'Happy indeed is he who has been able to discover the causes of things.'

Cardiff is a sea-port, and a great sea-port, and the Bristol Channel is a notable sea-fisheries centre of growing importance. The explorers and merchant venturers of the South-West of England are celebrated in history. What are you doing now in Cardiff to advance our knowledge of the ocean? You have here an important university centre and a great modern national museum, and either or both of these homes of research might do well to establish an oceanographical department, which would be an added glory to your city and of practical utility to the country. This is the obvious centre in Wales for a sea-fisheries institute for both research and education. Many important local movements have arisen from British Association meetings, and if such a notable scientific development were to result from the Cardiff meeting of 1920, all who value the advance of knowledge and the application of knowledge to industry would applaud your enlightened action.

But in a wider sense, it is not to the people of Cardiff alone that I appeal, but to the whole population of these Islands, a maritime people who owe everything to the sea. I urge them to become better informed in regard to our national sea-fisheries and take a more enlightened interest in the basal principles that underlie a rational regulation and exploitation of these important industries. National efficiency depends to a very great extent upon the degree in which scientific results and methods are appreciated by the people and scientific investigation is promoted by the Government and other administrative authorities. The principles and discoveries of science apply to aquiculture no less than to agriculture. To increase the harvest of the sea the fisheries must be continuously investigated, and such cultivation as is possible must be applied, and all this is clearly a natural application of the biological and hydrographical work now united under the science of Oceanography.

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ADDRESS

TO THE

MATHEMATICAL AND PHYSICAL SCIENCE SECTION

BY

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PRESIDENT OF THE SECTION.

The Organisation of Thought.

THE subject of this address is the organisation of thought, a topic evidently capable of many diverse modes of treatment. I intend [illegible] to give some account of that department of logical science with [illegible] my own studies have been connected. But I am anxious, if I can succeed in so doing, to handle this account so as to exhibit the relation with certain considerations which underlie general scientific activities.

It is no accident that an age of science [illegible] to an age of organisation. Organised thought is the basis of [illegible]. Organisation is the adjustment of diverse elements so that their mutual relations may exhibit some predetermined quality. An epic poem is a triumph of [illegible] that is to say, it is a triumph in the [illegible] it being a [illegible] poem. It is the successful organisation [illegible] sounds of words, associations of words, pictorial memories of diverse events and feelings ordinarily occurring in life, combined with a special narrative of great events: the whole so disposed as to excite emotions which, as defined by Milton, are simple, sensuous, and passionate. The number of successful epic poems is commensurate, or, rather, is inversely commensurate with the obvious difficulty of the task of organisation.

Science is the organisation of thought. But the example of the epic poem warns us that science is not any organisation of thought. It is an organisation of a certain definite type which we will endeavour to determine.

Science is a river with two sources, the practical source and the theoretical source. The practical source is the desire to direct our actions to achieve predetermined ends For example, the British nation, fighting for justice, turns to science, which teaches it the importance of compounds of nitrogen. The theoretical source is the desire to understand. Now I am going to emphasise the importance of theory in science. But to avoid misconception I most [illegible] state that I do not consider one source as in any sense nobler than the other, or intrinsically more interesting. I cannot see why it is nobler to strive to understand than to busy oneself with the right ordering of one's actions. Both have their bad sides; there are evil ends directing actions, and there are ignoble curiosities of the understanding.

The importance, even in practice, of the theoretical side of science arises from the fact that action must be immediate, and takes place under circumstances which are excessively complicated. If we wait for the necessities of action before we commence to arrange our ideas, in peace we shall have lost our trade, and in war we shall have lost the battle.

Success in practice depends on theorists who, led by other motives of exploration, have been there before, and by some good chance have hit upon the relevant ideas. By a theorist I do not mean a man who is up in the clouds,

but a man whose motive for thought is the desire to formulate correctly the rules according to which events occur. A successful theorist should be excessively interested in immediate events, otherwise he is not at all likely to formulate correctly anything about them. Of course, both sources of science exist in all men.

Now, what is this thought organisation which we call science? The first aspect of modern science which struck thoughtful observers was its inductive character. The nature of induction, its importance, and the rules of inductive logic have been considered by a long series of thinkers, especially English thinkers, Bacon, Herschel, J. S. Mill, Venn, Jevons, and others. I am not going to plunge into an analysis of the process of induction Induction is the machinery and not the product, and it is the product which I want to consider. When we understand the product we shall be in a stronger position to improve the machinery.

First, there is one point which it is necessary to emphasise. There is a tendency in analysing scientific processes to assume a given assemblage of concepts applying to nature, and to imagine that the discovery of laws of nature consists in selecting by means of inductive logic some one out of a definite set of possible alternative relations which may hold between the things in nature answering to these obvious concepts. In a sense this assumption is fairly correct, especially in regard to the earlier stages of science Mankind found itself in possession of certain concepts respecting nature—for example, the concept of fairly permanent material bodies—and proceeded to determine laws which related the corresponding percepts in nature. But the formulation of laws changed the concepts, sometimes gently by an added precision, sometimes violently. At first this process was not much noticed, or at least was felt to be a process curbed within narrow bounds, not touching fundamental ideas. At the stage where we now are, the formulation of the concepts can be seen to be as important as the formulation of the empirical laws connecting the events in the universe as thus conceived by us. For example, the concepts of life, of heredity, of a material body, of a molecule, of an atom, of an electron, of energy, of space, of time, of quantity, and of number. I am not dogmatising about the best way of getting such ideas straight. Certainly it will only be done by those who have devoted themselves to a special study of the facts in question. Success is never absolute, and progress in the right direction is the result of a slow, gradual process of continual comparison of ideas with facts. The criterion of success is that we should be able to formulate empirical laws, that is, statements of relations, connecting the various parts of the universe as thus conceived, laws with the property that we can interpret the actual events of our lives as being our fragmentary knowledge of this conceived interrelated whole.

But, for the purposes of science, what is the actual world? Has science to wait for the termination of the metaphysical debate till it can determine its own subject-matter? I suggest that science has a much more homely starting-ground. Its task is the discovery of the relations which exist within that flux of perceptions, sensations, and emotions which forms our experience of life. The panorama yielded by sight, sound, taste, smell, touch, and by more inchoate sensible feelings, is the sole field of its activity. It is in this way that science is the thought organisation of experience. The most obvious aspect of this field of actual experience is its disorderly character. It is for each person a *continuum*, fragmentary, and with elements not clearly differentiated. The comparison of the sensible experiences of diverse people brings its own difficulties. I insist on the radically untidy, ill-adjusted character of the fields of actual experience from which science starts. To grasp this fundamental truth is the first step in wisdom, when constructing a philosophy of science. This fact is concealed by the influence of language, moulded by science, which foists on us exact concepts as though they represented the immediate deliverances of experience. The result is that we imagine that we have immediate experience of a world of perfectly defined objects implicated in perfectly defined events which, as known to us by the direct deliverance of our senses, happen at exact instants of time, in a space formed by exact points, without parts and without

magnitude: the neat, trim, tidy, exact world which is the goal of scientific thought.

My contention is that this world is a world of ideas, and that its internal relations are relations between abstract concepts, and that the elucidation of the precise connection between this world and the feelings of actual experience is the fundam of scientific philosophy. The question which I am inviting you to : How does exact thought apply to the fragmentary, vague *continua* of experience? I am not saying that it does not apply, quite the contrary. But I want to know how it applies. The solution I am asking for is not a phrase however brilliant, but a solid branch of science, constructed with slow patience, showing in detail how the correspondence is effected.

The first great steps in the organisation of thought were due the practical source of scientific activity, without any admixture impulse. Their slow accomplishment was the cause and also the effect of the gradual evolution of moderately rational beings. I mean the formation of the concepts of definite material objects, of the determinate lapse of time, of simultaneity, of recurrence, of definite relative position, and of analogous fundamental ideas, which the flux of our experiences is mentally arranged for : in fact, the whole apparatus of common-sense thought. Consider in your mind some definite chair. The concept of that chair is simply the concept of all the interrelated experiences connected with that chair—namely, of the experiences of the folk who made it, of the folk who sold it, of the folk who have seen it or used it, of the man who is now experiencing a comfortable sense of support, combined with our expectations of an analogous future, terminated finally by a different set of experiences when the chair collapses and becomes fire-wood. The formation of that type of concept was a tremendous job, and zoologists and geologists tell us that it took many tens of millions of years. I can well believe it.

I now emphasise two points. In the first place, science is rooted in what I have just called the whole apparatus of common-sense thought. That is the *datum* from which it starts, and to which it must recur. We may speculate, if it amuses us, of other beings in other planets who have arranged analogous exper an entirely different conceptual code—namely, who have attention to different relations between their various But the task is too complex, too gigantic, to be revised in its You may polish up common sense, you may contradict it in detail, you may surprise it. But ultimately your whole task is to satisfy it.

In the second place, neither common sense nor science can proceed with their task of thought organisation without departing in some respect from the strict consideration of what is actual in experience. Think again of the chair. Among the experiences upon which its concept is based, I included our expectations of its future history. I should have gone further and included our all the possible experiences which in ordinary language we should of the chair which might have occurred. This is a difficult question, and I do not see my way through it. But in the construction of a theory of space and of time, there seem difficulties if we refuse to admit ideal

This imaginative f experiences, which, if they occurred, would be coherent with our actual experiences, seems fundamental in our lives. It is neither wholly arbitrary, nor yet fully determined. It is a vague background which is only made in part definite by isolated activities of thought. Consider, for example, our thoughts of the unseen flora of Brazil

Ideal experiences are closely connected with our imaginative reproduction of the actual experiences of other people, and also with our almost inevitable conception of ourselves as receiving our impressions from an external complex reality beyond ourselves. It may be that an adequate analysis of every source and every type of demonstrative proof of such a reality and of its nature. Indee to be doubted that this is the case. The precise elucidation of this question is the problem of metaphysics. One of the points which I am urging in this address is that the bas of science does not depend on the assumption of any of the conclusions of metaphysics; but that

both science and metaphysics start from the same give [illegible] immediate experience, and in the main proceed in opposite [illegible] diverse tasks.

For examp[illegible] inquires how our perceptions of the chair relate us to some true [illegible] gathers up these perceptions into a determinate class, adds to them ideal perceptions of analogous sort, which under assignable circumstances would be obtained, and this single concept of that set of perceptions is all that science needs; unless indeed you prefer that thought find its origin in some legend of those great twin brethren, the Cock and Bull.

My immediate problem is to inquire into the nature of the texture of science. Science is essentially logical. The nexus between its concepts is a logical nexus, and the grounds for its detailed assertions are logical grounds. King James said, 'No bishops, no king.' With greater confidence we can say, 'No logic, no science.' The reason for the instinctive dislike which most men of science feel towards the recognition of this truth is, I think, the barren failure of logical theory during the past three or four centuries. We may trace this failure back to the worship of authority which in some respects increased in the learned world at the time of the Renaissance. Mankind then changed its authority, and this fact temporally acted as an emancipation. But the main fact, and we can find complaints[1] of it at the very commencement of the modern movement, was the establishment of a reverential attitude towards any statement made by a classical author. Scholars became commentators on truths too fragile to bear translation. A science which hesitates to forget its founders is lost. To this hesitation I ascribe the barrenness of logic. Another reason for distrust of logical theory and of mathematics is the belief that deductive reasoning can give you nothing new. Your conclusions are contained in your premises, which by hypothesis are known to you.

In the first place this last condemnation of logic neglects the fragmentary, disconnected character of human knowledge. To know one premise on Monday, and another premise on Tuesday, is useless to you on Wednesday. Science is a permanent record of premises, deductions, and conclusions, verified all along the line by its correspondence with facts. Secondly, it is untrue that when we know the premises we also know the conclusions In arithmetic, for example, mankind are not calculating boys. Any theory which proves that they are conversant with the consequences of their assumptions must be wrong. We can imagine beings who possess such insight. But we are not such creatures. Both these answers are, I think, true and relevant. But they are not satisfactory. They are too much in the nature of bludgeons, too external. We want something more explanatory of the very real difficulty which the question suggests. In fact, the true answer is embedded in the discussion of our main problem of the relation of logic to natural science.

It will be necessary to sketch in broad outline some relevant features of modern logic. In doing so I shall try to avoid the profound general discussions and the minute technical classifications which occupy the main part of traditional logic. It is characteristic of a science in its earlier stages—and logic has become fossilised in such a stage—to be both ambitiously profound in its aims and trivial in its handling of details. We can discern four departments of logical theory. By an analogy which is not so very remote I will call these departments or sections the arithmetic section, the algebraic section, the section of general-function theory, the analytic section. I do not mean that arithmetic arises in the first section, algebra in the second section, and so on; but the names are suggestive of certain qualities of thought in each section which are reminiscent of analogous qualities in arithmetic, in algebra, in the general theory of a mathematical function, and in the analysis of the properties of particular functions.

The first section—namely, the arithmetic [illegible] with the relations of definite propositions to each other, just a [illegible] deals with definite numbers. Consider any definite proposition; call it 'p.' We conceive that there is always another proposition which is the direct contradictory to 'p'; call it 'not-p.' When we have got two propositions, p and q, we can form derivative

[1] *e.g.*, in 1551 by Italian schoolmen.

propositions from them, and from their contradictories. We can say, 'At least one of p or q is true, and perhaps both.' Let us call thi[illegible] 'p or q.' I may mention as an aside that one of the greatest living [illegible] is stated that this use of the word 'or'—namely, 'p or q' in the s[illegible] or both may be true—makes him despair of exact expression. We must brave his wrath, which is unintelligible to me.

We have thus got hold of four new propositions, namely, 'p or q,' and 'not-p or q,' and 'p or not-q,' and 'not-p or not-q.' Call these the set of disjunctive derivatives. There are, so far, in all eight propositions, p, not-p, q, not-q, and the four disjunctive derivatives. Any pair of these eight propositions can be taken, and substituted for p and q in the foregoing treatment. Thus each pair yields eight propositions, some of which may have been obtained before. By proceeding in this way we arrive at an unending set of propositions of growing complexity, ultimately derived from the two original propositions p or q. Of course, only a few are important. Similarly we can start from three propositions, p, q, r, or from four propositions, p, q, r, s, and so on. Any one of the propositions of these aggregates may be true or false. It has no other alternative. Whichever it is, true or false, call it the 'truth-value' of the [illegible]

[illegible] tion of logical inquiry is to settle what we know of the truth-values of these propositions, when we know the truth-values of some of them. The inquiry, so far as it is worth while carrying it, is not very abstruse, and the best way of [illegible] sults is a detail which I will not now consider. This inquiry for[illegible] stage

The next section of logic is the algebraic stage. Now, the difference between arithmetic and algebra is that in arithmetic definite numbers are considered, and in algebra symbols—namely, letters—are introduced which stand for any numbers. The idea of a number is also enlarged. These letters, standing for any numbers, are called sometimes variables and sometimes parameters. Their essential characteristic is that they are undetermined, unless, indeed, the algebraic conditions which they satisfy implicitly determine them. Then they are sometimes called unknowns. An algebraic formula with letters is a blank form. It becomes a determinate arithmetic statement when definite numbers are substituted for the letters. The importance [illegible] is a tribute to the study of form. Consider now the following [illegible]

The specific heat of mercury is 0·033.

This is a definite proposition which, with certain limitations, is true. But the truth-value of the proposition does not immediately concern us. Instead of mercury put a mere letter which is the name of some undetermined thing: we get,

The specific heat of x is 0·033.

This is not a proposition; it has been called by Russell a [illegible] function It is the logical analogy of an algebraic expression. Let [illegible] $f(x)$ for any propositional function.

We could also generalise still further, and say,

The specific heat of x is y.

We thus get another propositional function, $F(x, y)$ of two arguments x and y, and so on for any number of [illegible].

Now, consider $f(x)$. There [illegible] of values of x, for which $f(x)$ is a proposition, true or false. For values of x outside this range, $f(x)$ is not a proposition at all, and is neither true nor false. It may have vague suggestions for us, but it has no unit meaning of definite assertion. For example,

The specific heat of water is 0·033

is a proposition which is false; and

The specific heat of virtue is 0·033

is, I should imagine, not a proposition at all; so that it is neither true nor false, though its component parts raise various associations in our minds. This

range of values, for which $f(x)$ has sense, is called the 'type' of the argument x.

But there is also a range of values of x for which $f(x)$ is a true proposition. This is the class of those values of the argument which *satisfy* $f(x)$. This class may have no members, or, in the other extreme, the class may be the whole type of the arguments.

We thus conceive two general propositions respecting the indefinite number of propositions which share in the same logical form, that is, which are values of the same propositional function. One of these propositions is,

$f(x)$ yields a true proposition for each value of x of the proper type;

the other proposition is,

There is a value of x for which $f(x)$ is true.

Given two, or more, propositional functions $f(x)$ and $\phi(x)$ with the same argument x, we form derivative propositional functions, namely,

$$f(x) \text{ or } \phi(x),\ f(x) \text{ or not-}\phi(x),$$

and so on with the contradictories, obtaining, as in the arithmetical stage, an unending aggregate of propositional functions. Also each propositional function [illegible] general propositions. The theory of the interconnection between the [illegible] of the general propositions arising from any such aggregate of propositional functions forms a simple and elegant chapter of mathematical logic.

In this algebraic section of logic the theory of types crops up, as we have already noted. It cannot be neglected without the introduction of error. Its theory has to be settled at least by some safe hypothesis, even if it does not go to the philosophic basis of the question. This part of the subject is obscure and difficult, and has not been finally elucidated, though Russell's brilliant work has opened out the subject.

The final impulse to modern logic comes from the independent discovery of the importance of the logical variab[illegible] and Peano. Frege went further than Peano, but by an unfortunate [illegible] rendered his work so obscure that no one fully recognised his meaning who had not found it out for himself. But the movement has a large history reaching back to Leibniz and even to Aristotle. Among English contributors are De Morgan, Boole, and Sir Alfred Kempe; their work is of the first rank.

The third logical section is the stage of general-function theory. In logical language [illegible] in this stage the transition from intension to extension, and [illegible] theory of denotation. Take the propositional function $f(x)$. There is the class, or range of values for x, whose members satisfy $f(x)$. But the same range may be the class whose members satisfy another [illegible] function $\phi(x)$. It is necessary to investigate how to indicate [illegible] a way which is indifferent as between the various propositional functions which are satisfied by any member of it, and of it only. What has to be done is to analyse the nature of propositions about a class—namely, th[illegible] whose truth-values depend on the class itself and not on the [illegible]ing by which the class is indicated.

Furthermore, there are propositions about alleged individuals indicated by descriptive phrases: for example, propositions about 'the present King of England,' who does exist, and 'the present Emperor of Brazil,' who does not exist. More complicated, but analogous, questions involving propositional functions of two variables involve the notion of 'correlation,' just as functions of one argument involve classes. Similarly functions of three arguments yield three-cornered correlations, and so on. This logical section is one which Russell has made peculiarly his own by work which must always remain fundamental. I have called this the section of functional theory, because its ideas are essential to the construction of logical denoting functions which include as a special case ordinary mathematical functions such as sine, logarithm, &c. In each of these three stages it will be necessary gradually to introduce an appropriate symbolism, if we are to pass on to the fourth stage.

The fourth logical section, the analytic stage, is concerned with the investigation of the properties of special logical constructions, that is, of classes and

correlations of special sorts. The whole of mathematics is included here. So the section is a large one. In fact, it is mathematics, neither more nor less. But it includes an analysis of mathematical ideas not hitherto included in the scope of that science, nor, indeed, [illegible] at all. The essence of this stage is construction. It is by mean[illegible] constructions that the great framework of [illegible]atics, comprising the theories of number, quantity, time, and spac[illegible]

It is impossible even in brief outline to explain how mathematics is developed from the concepts of class and correlation, including many-cornered correlations, which are established in the third section. I can only allude to the headings of the process which is fully developed in the work, 'Mathematica Principia,' by Mr. Russell and myself. There are in this process of development seven special sorts of correlations which are of peculiar interest. The first sort comprises one-to-many, many-to-one, and one-to-one correlations. The second sort comprises serial relations, that is, correlations by which the members of some field are arranged in a serial order, so that, in the sense defined by the relation, any member of the field is either before or after any other member. The third class comprises inductive relations, that is, correlations on which the theory of mathematical induction depends The fourth [illegible] tive relations, which are required for the general theory of [illegible] and elsewhere. It is in connection with such relations that [illegible] cative axiom arises for consideration. The fifth class comprises vector relations, from which the theory of quantity arises. The sixth class comprises ratio relations, which interconnect number and quantity. The seventh class comprises three-cornered and four-cornered relations which occur in Geometry.

A bare enumeration of technical names, such as the above, is not very illuminating, though it may help to a comprehension of the demarcations of the subject. Please remember that the names are technical names, meant, no doubt, to be suggestive, but used in strictly defined senses. We have suffered much from critics who consider it sufficient to criticise our procedure on the slender basis of a knowledge of the dictionary meanings of such terms. For example, a one-to-one correlation depends on the notion of a class with only one member, and this notion is defined without appeal to the concept of the number one. The notion of di[illegible] is all that is wanted. Thus the class a has only one member, if (1) [illegible] of values of x which satisfies the propositional function,

x is not a member of a,

is not the whole type of relevant values of x, and (2) the propositional function,

x and y are members of a, and x is diverse from y,

is false, whatever be the values of x and y in the relevant type.

Analogous procedures are obviously possible for higher finite cardinal members. Thus, step by step, the whole cycle of current mathematical ideas is capable of logical definition. The process is detailed and laborious, and, like all science, knows nothing of a royal road of airy phrases. The essence of the process is, first to construct the notion in terms of the forms of propositions, that is, in terms of the relevant [illegible] functions, and secondly to prove the fundamental truths which hol[illegible] notion by reference to the results obtained in the algebraic section of logic.

It will be seen that in this process the whole apparatus of special indefinable mathematical concepts, and special *a priori* mathematical premises, respecting number, quantity, and space, has vanished. Mathematics is merely an apparatus for analysing the deductions which can be drawn from any particular premises, supplied by common sense, or by more refined scientific observation, so far as these deductions depend on the forms of the propositions. Propositions of certain forms are continually occurring in thought. Our existing mathematics is the analysis of deductions, which concern those forms and in some way are important, either from practical utility or theoretical interest. Here I am speaking of the science as it in fact exists. A theoretical definition of mathematics must include in its scope any deductions depending on the mere forms

of propositions. But, of course, no one would wish to develop that part of mathematics which in no sense is of importance.

This hasty summary of logical ideas suggests some reflections. The question arises, How many forms of propositions are there? The answer is, an unending number. The reason for the supposed sterility of logical science can thus be discerned. Aristotle founded the science by conceiving the idea of the form of a proposition and by conceiving deduction as taking place in virtue of the forms. But he confined propositions to four forms, now named A, I, E, O. So long as logicians were obsessed by this unfortunate restriction, real progress was impossible. Again, in their theory of form, both Aristotle and subsequent logicians came very near to the theory of the logical variable. But to come very near to a true theory, and to grasp its precise application, are two very different things, as the history of science teaches us. Everything of importance has been said before by somebody who did not discover it.

Again, one reason wh[illegible]tions are not obvious is that logical form is not a subject which [illegible]rs into thought. Common-sense deduction probably moves by [illegible] from concrete proposition to concrete proposition, guided by some habitual association of ideas. Thus common sense fails in the presence of a wealth of material.

A more important question is the relation of induction, based on observation, to deductive logic. There is a tradition of opposition between adherents of induction and of deduction. In my view, it would be just as sensible for the two ends of a worm to quarrel. Both observation and deduction are necessary for any knowledge worth having. We cannot get at an inductive law without having recourse to a propositional function For example, take the statement of observed fact,

This body is mercury, and its specific heat is 0·033.

The propositional function is formed,

Either x is not mercury, or its specific heat is 0·033.

The inductive law is the assumption of the truth of the general proposition, that the above propositional function is true for every value of x in the relevant type.

But it is objected that this process and its consequences are so simple that an elaborate science is out of place. In the same way, a British sailor knows the salt sea when he sails over it. What, then, is the use of an elaborate chemical analysis of sea-water? There is the general answer, that you cannot know too much of methods which you always employ; and there is the special answer, that logical forms and logical implications are not so very simple, and that the whole of mathematics is evidence to this effect

One great use of the study of logical method is not in the region of elaborate deduction, bu[illegible] the study of the formation of the main concepts of science. [illegible]y, for example. What are the points which compose space? Euclid tells us that they are without parts and without magnitude. But how is the notion of a point derived from the sense-perceptions from which science starts? Certainly points are not direct deliverances of the senses. Here and there we may see or unpleasantly feel something suggestive of a point But this is a rare phenomenon, and certainly does not warrant the conception of space as composed of points. Our knowledge of space [illegible], not based on any observations of relations between points. It [illegible] experience of relations between bodies. Now a fundamental space relation between bodies is that one body may be part of another. We are tempted to define the 'whole and part' relation by saying that the points occupied by the part are some of the points occupied by the whole. But 'whole and part' being more fundamental than the notion of 'point,' this definition is really circular and vicious.

We accordingly ask whether any other definition of 'spatial whole and part' can be given. I think that it can be done in this way, though, if I be mistaken, it is unessential to my general argument. We have come to the conclusion that an extended body is nothing else than the class of perceptions of it by all its percipients, actual or ideal. Of course, it is not any class of perceptions, but a certain definite sort of class which I have not defined here, except by

the vicious method of saying that they are [illegible] of a body. Now, the perceptions of a part of a body are among the [illegible] which compose the whole body. Thus two bodies a and b are both [illegible] perceptions; and b is part of a when the class which is b is contained in the class which is a. It immediately follows from the logical form of this definition that if b is part of a, and c is part of b, then c is part of a. Thus the relation 'whole to part' is transitive. Again, it will be convenient to allow that a body is part of itself. This is a mere question of how you draw the definition. With this understanding, the relation is reflexive. Finally, if a is part of b, and b is part of a, then a and b must be identical. These [illegible] of 'whole and part' are not fresh assumptions, they follow from [illegible] form of our definition.

One assumption has to be made if we assume the ideal infinite divisibility of space. Namely, we assume that every class of perceptions which is an extended body contains other classes of perceptions which are extended bodies diverse from itself. This assumption makes rather a large draft on the theory of ideal perceptions. Geometry vanishes unless in some form you make it The assumption is not peculiar to my exposition.

It is then possible to define what we mean by a point. A point is the class of extended objects which, in ordinary language, contain that point. The definition, without presupposing the idea of a point, is rather elaborate, and I have not now time for its statement.

The advantage of introducing points into Geometry is the simplicity of the logical expression of their mutual relations. For science, simplicity of definition is of slight importance, but simplicity of mutual relations is essential. Another example of this law is the way physicists and chemists have dissolved the simple idea of an extended body, say of a chair, which a child understands, into a bewildering notion of a complex dance of molecules and atoms and electrons and waves of light. They have thereby gained notions with simpler logical relations.

Space as thus conceived is the exact formulation of the properties of the apparent space of the common-sense world of experience. It is not necessarily the best mode of conceiving the space of the physicist. The one essential requisite is that the correspondence between the common-sense world in its space and the physicists' world in its space should be definite and reciprocal.

I will now break off the exposition of the function of logic in connection with the science of natural phenomena. I have endeavoured to exhibit it as the organising principle, analysing the derivation of the concepts from the [illegible], examining the structure of the general propositions which are the assumed laws of nature, establishing their relations to each other in respect to reciprocal implications, deducing the phenomena we may expect under given circumstances.

Logic, properly used, does not shackle thought. It gives freedom and, above all, boldness. Illogical thought hesitates to draw conclusions, because it never knows either, what it means, or what it assumes, or how far it trusts its own assumptions, or what will be the effect of any modification of assumptions. Also the mind untrained in that part of constructive logic which is relevant to the subject in hand will be ignorant of the sort of conclusions which follow from various sorts of [illegible] 'll be correspondingly dull in divining the inductive laws. [illegible] training in this relevant logic is, undoubtedly, to ponder with an active mind over the known facts of the case, directly observed. But where elaborate deductions are possible, this mental activity requires for its full exercise the direct study of the abstract logical relations. This is applied mathematics.

Neither logic without observation, nor observation without logic, can move one step in the formation of science We may conceive humanity as engaged in an internecine conflict between youth and age. Youth is not defined by years, but by the creative impulse to make something. The aged are those who, before all things, desire not to make a mistake. Logic is the olive branch from the old to the young, the wand which in the hands of youth has the magic property of creating science.

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British Association for the Advancement of Science.

SECTION B: NEWCASTLE-ON-TYNE, 1916.

ADDRESS

TO THE

CHEMICAL SECTION

BY

PROFESSOR G. G. HENDERSON, D.Sc., LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

FOR the third time in succession the Section meets under the shadow of the war cloud, but there is some slight consolation for the indescribable suffering and sorrow which have been imposed upon millions of our fellow creatures in the hope and belief that this cloud also may have a silver lining. I[illegible] exaggeration to say that nothing less than such an upheaval of [illegible] and traditions as has been caused by the war would have suffice[illegible] British nation from the state of apathy towards science with which it has been fatuously contented in the past. Now, however, the sleeper has at least stirred in his slumber. The Press bears witness, through the appearance of innumerable articles and letters, that the people of this country, and even the politicians, have begun t[illegible] the dangers which will inevitably result from a continuance of [illegible] attitude, and to understand that in peace, as in war, civilisation is at a tremendous disadvantage in the struggle for existence unless armed by science, and that the future prosperity of the Empire is ultimately dependent upon the progress of science, and very specially of chemistry. If, as one result of the war, our people are led to [illegible] the value of scientific work, then perhaps we shall not have paid too [illegible]ice, high although the price must be. As concerns our own branch of science, we cannot rest satisfied with anything less than full recognition of the fact that chemistry is a profession of fundamental importance, and that the chemist is entitled to a position in no respect inferior to that of a member of any of the other learned professions.

Reference to the Annual Reports of the Association shows that former Presidents of the Section have availed themselves to the full of the latitude permitted in the choice of a subject for their Address, and that some have even established the precedent of dispensing with an Address altogether. On the present occasion a topic for discussion seems to be clearly indicated by the circumstances in which we stand, because, since the outbreak of the war, chemists have been giving more earnest consideration than before to the present position and future prospects of the chemical industry of this country. It will, therefore, not be inappropriate if I touch upon some aspects of this question. even although unable to add much to what is, or ought to be, common knowledge.

The period which has elapsed since the last meeting of the Section in Newcastle has witnessed truly remarkable progress in every branch of pure and applied chemistry. For fully fifty years previous to that meeting the attention of the great majority of chemists had been devoted to organic chemistry, but since 1885 or [illegible] whilst the study of the compounds of carbon has been pursued [illegible] energy and success, it has no longer so largely monopolised t[illegible] f investigators. Interest in the other elements,

which had been to some extent neglected on account of the fascinations of carbon, has been revived with t[illegible]s, for not only has our knowledge of these elements been [illegible] but their number also has been notably increased by the discovery of two groups of simple substances possessed of new and remarkable properties—the inert gases of the argon family and the radio-active elements. In addition, the bonds between mathematics a[illegible] on the one hand and chemistry on the other have been drawn [illegible] the effect that the department of our science known as physical chemistry has now assumed a position of first-rate importance. With the additional light provided by the development and application of physico-chemical theory and methods, we are beginning to gain some insight into such [illegible] as the relation between physical properties and chemical [illegible]tructure of molecules and even of atoms, and the mechanics of chemical change; our outlook is being widened, and our conceptions rendered more precise. Striking advances have also been made in other directions. The extremely difficult problems which confront the bio-chemist are being gradually overcome, thanks to the indefatigable labours of a band of highly skilled observers, and the department of biological chemistry has been established on a firm footing through the encouraging results obtained within the period under review. Further, within the last few years many of our ideas have been subjected to a revolutionary change through the study of the radio-active elements, these elusive substances which occur in such tantalisingly minute quantities, and of which some appear so reluctant to exist in a free and independent state that they merge their identity in that of another and less retiring relative within an interval of time measured by seconds. In truth, if a Rip Van Winkle among chemists were to awake now after a slumber of thirty years, his amazement on coming into contact with the chemistry of to-day would be beyond words.

The more purely scientific side of our science can claim no monopoly in progress, for applied chemistry, in every d[illegible], has likewise advanced with giant strides, mainly of course throug[illegible] of the results of scientific research to industrial purposes. An attempt to sketch in the merest outline the recen[illegible] of applied chemistry would, I fear, exhaust your patience, but I [illegible] in passing some of the main lines of advance. Many of the m[illegible] results in the field of modern chemical industry have been obtained by taking advantage of the powers we now possess to carry out operations economically both at very high and at very low temperatures, and by the employment on the [illegible]scale of electrolytic and catalytic methods of production. Thank[illegible] invention of the dynamo, the technologist is now able to utilise electrical energy both for the production of high temperatures in the different types of electric furnace and for electrolytic processes of the most varied description. Among the operations carried out with the help of the electric furnace may be mentioned the manufacture of graphite, silicon, and phosphorus; of chromium and other metals; of carbides, silicides, and nitrides; and the smelting and refining of iron and steel. Calcium carbide claims a [illegible] place in the list, in the first place because of the ease with which i[illegible] acetylene, which is not only used as an illuminant, and, in the oxy-acetylene burner, as a means of producing a temperature so high that the cutting and welding of steel is now a comparatively simple matter, but also promises to serve as the starting-point for the industrial synthesis of acetaldehyde and many other valuable organic compounds. Moreover, calcium carbide is readily converted in the electric furnace into calcium cyanamide, which is employed as an efficient fertiliser in place of sodium nitrate or ammonium sulphate, and as a source of ammonia and of alkali cyanides. Among the silicides carborundum is increasingly used as an abrasive and a refractory material, and calcium silicide, which is now a commercial product, forms a constituent of some blasting explosives. The Serpek process for the preparation of alumina and ammonia, by the formation of aluminium nitride from beauxite in the electric furnace and its subsequent decomposition by caustic soda, should also be mentioned. Further, the electric furnace has made possible the manufacture of silica apparatus of all kinds, both for the laboratory and the works, and of alundum ware, also used for operations at

high temperature Finally, the first step in the manufacture of nitric acid and of nitrites from air, now in operation on a very large scale, is the combustion of nitrogen in the electric arc.

In other industrial [illegible] high temperature which is necessary is obtained by the help of [illegible] or the oxy-acetylene flame, the former being used, amongst other purposes, in a small but I believe profitable industry, the manufacture of synthetic rubies, sapphires, and spinels. Also, within a comparatively recent period, advantage has been taken of the characteristic properties of aluminium, now obtainable at a moderate price, in the various operations classed under the heading alumino-thermy, the most important being the reduction of refractory metallic oxides, although, of course, thermite is useful for the production of high temperatures locally.

The modern methods of liquefying gases, which have been developed within the period under review, have rendered possible research work of absorbing interest on the effect of very low temperatures on the properties and chemical activity of many substances, and have been applied, for instance, in separating from one another the members of the argon family, and in obtaining ozone in a state of practical purity. Moreover, industrial applications of these methods are not lacking, amongst which I may mention the separation of nitrogen and oxygen from air, and of hydrogen from water-gas—processes which have helped to make these elements available for economic use on the large scale.

Electrolytic methods are now extensively employed in the manufacture of both inorganic and organic substances, and older processes are being displaced by these modern rivals in steadily increasing number. It is sufficient to refer to the preparation of sodium, magnesium, calcium, and aluminium, by electrolysis of fused compounds of these metals; the refining of iron, copper, silver, and gold; the extraction of gold and nickel from solution; the recovery of tin from waste tin-plate; the preparation of caustic alkalis (and simultaneously of chlorine), of hypochlorites, chlorates, and perchlorates, of hydrosulphites, of [illegible] and [illegible] persulphates and percarbonates; the regene- [illegible] mic [illegible] salts; the preparation of hydrogen and oxygen As regards organic compounds we find chiefly in use electrolytic methods of reduction, which are specially effective in the case of many nitro compounds, and of oxidation, as for instance the conversion of anthracene into anthraquinone At the same time a number of other compounds, for example iodoform are also prepared electrolytically.

Within recent years there have been great advances in the application of catalytic methods to industrial purposes. Some processes of this class have, of course, been in use for a considerable time, for example the Deacon chlorine process and the contact method for the manufacture of sulphuric acid, whilst the [illegible] of phthalic anhydride (largely used in the synthesis of indigo and other dyestuffs), by the oxidation of naphthalene with sulphuric acid with the assistance of mercuric sulphate as catalyst, is no novelty. More recent are the contact methods of obtaining ammonia by the direct combination of nitrogen and hydrogen, and of oxidising ammonia to nitric acid—both of which are said to be in operation on a very large scale in Germany. The catalytic action of metals, particularly nickel and copper, is utilised in processes of hydrogenation—for example, the hardening of fats, and of dehydrogenation, as in the preparation of acetaldehyde from alcohol, and such metallic oxides as alumina and thoria can be used for processes of dehydration—*e.g.*, the [illegible] of ethylene or of ether from alcohol. Other catalysts employed in [illegible] processes are titanous chloride in electrolytic reductions and cerous sulphate in electrolytic oxidations of carbon compounds, gelatine in the preparation of hydrazine from ammonia, sodium in the synthesis of rubber, &c.

Other advances in [illegible] chemistry include the preparation of a number of the rarer el[illegible] compounds, which were hardly known thirty years ago, but which now find commercial applications Included in this category are titanium, vanadium, tungsten, and tantalum, now used in metallurgy or for electric-lamp filaments; thoria and ceria in the form of mantles for incandescent lamps; pyrophoric alloys of cerium and other metals; zirconia, which appears to be a most valuable refractory material; and compounds of radium and of mesothorium, for medical use as well as for research. Hydrogen,

together with oxygen and nitrogen, are in demand for synthetic purposes, and the first also for [illegible] craft. Ozone i[illegible] for sterilising water and as [illegible]gent, for examp[illegible] of vanillin from isoeugenol [illegible] now [illegible] in concentrated solution, and the [illegible]umber [illegible]also utilised in many different ways. The [illegible]bonic, and persulphuric—or their salts are employed [illegible] purposes, and sodium hydrosulphite is much in [illegible]t—*e.g.* in dyeing with indigo. Hydroxylamine and hydrazine are used in considerable quantity, and the manufacture of cyanides by one or other of the modern methods has become quite an important industry, mainly owing to the use of the alkali salts in the cyanide process of gold extraction. These remarkable compounds the metallic carbonyls have been investigated, and nickel carbonyl is employed on the commercial scale in the extraction of the metal. Fine chemicals for analysis and research are now supplied, as a matter of course, in a state of purity rarely attained a quarter of a century ago.

In the organic chemical industry similar continued progress is to be noted. Accessions are constantly being made to the already enormous list of synthetic dyes, not only by the addition of new members to existing groups, but also by the discovery of entirely new classes of tinctorial compounds; natural indigo seems doomed to share the fate of alizarine from madder and to be ousted by [illegible] of which, moreover, a number of useful derivatives are also [illegible] drugs of all kinds—antipyrine and phenacetin, sulphonal and veronal, novacain and β-eucaine, salol and aspirin, piperazine and adrenaline, atoxyl and salvarsan—are produced in large quantities, as also are many synthetic perfumes and flavouring materials, such as ionone, heliotropine, and vanillin Cellulose in the form of artificial silk is much used as a new textile material synthetic camphor is on the market, synthetic rubber is said to be produced in considerable quantity; and the manufacture of materials for photographic work and of organic compounds for research purposes is no small part of the industry. However, it would serve no useful purpose to extend this catalogue, which might be done almost indefinitely.

British chemists are entitled to regard with satisfaction the part which they have taken in the development of scientific chemistry during the last three decades, as in the past, but with respect to the progress of industrial chemistry it must be regretfully admitted that, except in isolated cases, we have failed to keep pace with our competitors Consider a single example. Although there still remain in South America considerable deposits of sodium nitrate which can be worked at a profit, it is clear that sooner or later other sources of nitric acid must be made available. The synthetic production of nitric acid from the air is now a commercial success; several different processes are in operation abroad, and Germany is reported to be quite independent of outside supplies. Electrical energy, upon the cost of which the success of the process largely depends, can be produced in this country at least as cheaply as in Germany, and yet we have done nothing in the matter, unless we count as something the appointment of a committee to consider possibilities. This case is only too typical of many others. A number of different causes have contributed to bring about this state of affairs, and the responsibility for it is assigned by some to the Government, by others to the chemical manufacturers, and by still others to the professors of chemistry. I think, however, it will be generally admitted that the root of the matter is to be found in the general ignorance of and indifference to the methods and results of scientific work which characterises the people of this country. For many years past our leaders in science have done all that lay in their power to awaken the country to the inevitable and deplorable results of this form of 'sleeping sickness,' but hitherto their reception has been much the same as that accorded to the hero of 'The Pilgrim's Progress,' as depicted in the following passage:—

'He went on thus, even until he came at a bottom where he saw, a little out of the way, three Men fast asleep with Fetters upon their heels.'

'The name of the one was *Simple*, another *Sloth*, and the third *Presumption*.'

'Christian, then seeing them in this case, went to them, if peradventure he might awaken them. And cried, You are like them that sleep on the top of a

Mast, for the Dead Sea is under you, a Gulf that hath no bottom. Awake therefore and come away; be willing also, and I will help you off with your irons. He also told them. If he that goeth about like a *Roaring Lion* comes by, you will certainly become a prey to his teeth.'

'With that they lookt upon him, and began to reply in this sort : *Simple* said, *I see no danger*; *Sloth* said, *Yet a little more sleep*; and *Presumption* said, *Every Vat must stand upon his own bottom.* And they lay down to sleep again, and *Christian* went on his way.'

I believe that a brighter day is dawning, and that, if only we rise to the occasion *now*, chemistry in this country will attain the position of importance which is its due. Meantime it is of no avail to lament lost opportunities or to indulge in unprofitable recrimination; on the contrary, it should be our business to find a remedy for the 'arrested development' of our chemical industry, and the task of establishing remedial measures should be taken in hand by the State, the universities and the chemical manufacturers themselves. As regards another very large group of interested persons, the consumers of chemical products, or in other words the nation as a whole, it is surely not too much to expect that they have been taught by the course of events since the outbreak of the war the folly of depending solely upon foreign and possibly hostile manufacturers, even although fiscal and other advantages may enable the alien to undersell the home producer. Considering that the future prosperity of the Empire depends largely upon the well-being of its chemical industries, it is simply suicidal to permit these to be crippled or even crushed out of existence by competition on unequal terms.

The Government has taken a most significant step in advance by appointing an Advisory Council for Scientific and Industrial Research and providing it with funds; incidentally, in so doing, it has redressed the past failure of the State to afford adequate support to scientific work. The Advisory Council has lost no time in getting to work and has already taken steps to allocate grants in support of a number of investigations of first-rate importance to industry. In order to be in a position to do justice to the branches of industry concerned in proposed researches which have been submitted by institutions and individuals, it has decided to appoint standing committees of experts and has already constituted strong Committees in Mining, Metallurgy, and in Engineering; a Committee in Chemistry will no doubt be appointed in due course. The Council also makes the gratifying intimation that the training of an adequate supply of research workers will be an important part of its work.

It is safe to prophesy that the money expended by the Advisory Council will sooner or later yield a goodly return, and this justifies the hope that the Government will not rest satisfied with their achievement, but will take further steps in the same direction. This desire for continued action finds strong support in the Recommendations made by a Sub-Committee of the Advisory Committee to the Board of Trade on Commercial Intelligence, which was appointed to report with respect to measures for securing the position, after the war, of certain branches of British industry. Of these recommendations quote the following :—'

1. *Scientific Industrial Research and Training.* (*a*) Larger funds should be placed at the disposal of the new Committee of the Privy Council, and also of the Board of Education, for the promotion of scientific and industrial training. (*b*) The universities should be encouraged to maintain and extend research work devoted to the main industry or industries located in their respective districts, and manufacturers engaged in these industries should be encouraged to co-operate with the universities in such work, either through their existing trade associations or through associations specially formed for the purpose. Such associations should bring to the knowledge of the universities the difficulties and needs of the industries, and give financial and other assistance in addition to that afforded by the State. In the case of non-localised industries trade associations should be advised to seek, in respect of centres for research, the guidance of the Advisory Committee of the Privy Council. (*c*) An authoritative record of consultant scientists, chemists and engineers, and of persons engaged in industrial research, should be established and maintained by some suitable Government Department for the use of manufacturers only.'

'2. *Tariff Protection.* Where the national supply of certain manufactured articles which are of vital importance to the national safety or are essential to other industries has fallen into the hands of manufacturers or traders outside this country, British manufacturers ready to undertake the manufacture of such articles in this country should be afforded sufficient tariff protection to enable them to maintain such production after the war.' (It is also recommended by the Sub-Committee that in view of the threatened dumping of stocks which may be accumulated in enemy countries, the Government should take such steps as would prevent the position of industries, likely to be affected, being endangered after the war.)

'3. *Patents.* (*a*) The efforts which have been made to secure uniformity of Patent Law throughout the Empire should be continued. (*b*) The provisions of the law as to the compulsory working of patents in the United Kingdom should be more rigorously enforced, and inspectors should be appointed to secure that such working is complete and not only partial.'

The adoption by the Government of these weighty recommendations would go far to establish British chemical industry on a secure basis, and would undoubtedly lead to the extension of already existing branches and the establishment of new ones. Meanwhile, the Australian Government has set an example which might be followed with great advantage. Shortly after the British scheme for the development of scientific and industrial research under the auspices of the Advisory Council had been made public, the Prime Minister of Australia determined to do still more for the Commonwealth, with the object of making it independent of German trade and manufactures after the conclusion of the war. He therefore [illegible] a committee representative of the State Scientific Departments, the universities, and industrial interests, and within a very short period the committee produced a scheme for the establishment of a Commonwealth Institute of Science and Industry. The Institute is to be governed by three directors, two of whom will be scientific men of high standing, while the third will be selected for proved ability in business. The directors are to be assisted by an Advisory Council composed of nine representatives of science and of industry; these representatives are to seek information, advice, and assistance from specialists throughout Australia. The chief functions of the Institute are (1) To ascertain what industrial problems are most pressing and most likely to yield to scientific experimental investigation, to seek out the most competent men to whom such research may be entrusted, and to provide them with all the necessary appliances and assistance. (2) To build up a bureau of scientific and industrial information, which shall be at the service of all concerned in the industries and manufactures of the Commonwealth. (3) To erect, staff, and control special research laboratories, the first of which will probably be a physical laboratory somewhat on the lines of our National Physical Laboratory. Other functions of the Institute are the co-ordination and direction of research and experimental work with a view to the prevention of undesirable overlapping of effort, the recommendation of grants of the Commonwealth Government in aid of pure scientific research in existing institutions, and the establishment and award of industrial research fellowships.

This admirable scheme is more comprehensive and more generous than that of our Government, but it could be rivalled without much difficulty. We already possess an important asset in the National Physical Laboratory, and there now exists the Advisory Council with its extensive powers and duties. What is lacking in our scheme, so far as chemistry is concerned, could be made good, firstly, by providing the Advisory Council with much larger funds, and, secondly, by the establishment of a National Chemical [illegible] institute for research in pure and applied chemistry—or by assisting the development of research departments in our universities and technical colleges (as is now being done in America), or, better still, by moving in both directions. With respect to the second alternative, I do not mean to suggest that research work is neglected in the chemistry departments of any of our higher institutions; what I plead for is the provision of greater facilities for the prosecution of [illegible] not only in pure but also in applied chemistry. As things are at present [illegible] professors and lecturers are for the most part so much occupied in teaching and in administration as to be unable to devote time uninterruptedly

to research work, which demands above all things continuity of effort. The ideal remedy would be the institution of research professorships, but, failing this, the burden of teaching and administrative work should be lightened by appointing larger staffs.

It has been suggested by Dr. Forster that the State could render assistance to chemical industry in another way, namely, by the formation of a Chemical Intelligence Department of the Board of Trade, which should be concerned with technical, commercial, and educational questions bearing upon the industry. Under the first head the proposed Department would have the duty (*a*) of collecting, tabulating, and distributing all possible information regarding chemical discoveries, patents, and manufacturing processes, and (*b*) of presenting problems for investigation to research chemists, of course under proper [illegible] and with suitable remuneration. The more strictly commercial side of the Department's activities would be concerned with the classification of the resources of the Empire as regards raw materials, and of foreign chemical products in respect of distribution [illegible] the world, with ruling prices, tariffs, cost of transport, and if [illegible] of production. On the educational side it is suggested that the Department should collect data regarding opportunities for chemical instruction and research in various parts of the Empire, and should consider possible improvements and extensions of these. The Department would of course be in charge of a highly trained chemist, with a sufficient number of chemical assistants.

This proposal, which has been widely discussed and on the whole very favourably received by chemists, has much to recommend it; to mention only one point, the unrivalled resources of the Board of Trade would facilitate the acquisition of information which might otherwise be difficult to obtain, or which would not be disclosed except to a Government Department. The principal objections which have been raised are based upon the fear that the proposed Department, however energetic and enterprising it might be at the start, would soon be so helplessly gagged and bound down by departmental red tape as to become of little or no service. This danger, however, could be obviated to a great extent by the institution of a strong Advisory Committee, representative of and elected by the Societies concerned with the different branches of chemistry, which would keep closely in touch with the Chemical Intelligence Department on the one hand and with the industry on the other, and which would act as adviser of the permanent scientific staff of the Department There is, I fear, little chance of seeing Dr. Forster's proposal carried into effect unless all the Societies concerned move actively and unitedly in the matter; they must do the pioneer work and must submit a definite scheme to the Government, if the desired result is to be attained. In the not improbable contingency that the Board of Trade will decline to take action, I trust that the scheme for the establishment of an Information Bureau—on lines similar to but somewhat less wide-reaching than those which I have just indicated—which has been under the careful consideration of the Council of the Society of Chemical Industry, will be vigorously prosecuted. Difficulties, chiefly financial, stand in the way, but these are not insuperable, especially if the sympathy and support of the Government can be enlisted.

Unless the conditions and methods which have ruled in the past are greatly altered it is hardly possible to hope that the future prospects of our chemical industry will be bright; it is essential that the representatives of the industry should organise themselves in their own interest and co-operate in [illegible] common enemy. More than ever is this the case when, as we are [illegible] three different groups of German producers of dyes, drugs, and fine chemicals, who own seven large factories, have formed a combination with a capital of more than 11,000,000*l*., and with other assets of very great value in the shape of scientific, technical, and financial efficiency. Hence it is eminently satisfactory to be able to record the active progress of a movement, originated by the Chemical Society, which has culminated in the formation of an Association of British Chemical Manufacturers. The main objects of the Association are to promote co-operation between British chemical manufacturers; to act as a medium for placing before the Government and Government officials the views of manufacturers upon matters affecting the chemical industry; to develop

technical organisation and promote industrial research; to keep in touch with the progress of chemical knowledge and to facilitate the development of new British industries and the extension of existing ones; and to encourage the sympathetic association of British manufacturers with the various universities and technical colleges.

Needless to say, the progress of this important movement will be assisted by everyone who is interested, either directly or indirectly, in the welfare of our chemical industry, and, moreover, the support of the scientific societies will not be lacking, for, as the result of a conference convened by the President and Council of the Royal Society, a Conjoint Board of Scientific Societies has been constituted, for the furtherance of the following objects:—Promoting the co-operation of those interested in pure or applied science; supplying a means whereby scientific opinion may find effective expression on matters relating to science, industry, and education; taking such action as may be necessary to promote the application of science to our industries and to the service of the nation; and discussing scientific questions in which international co-operation seems advisable.

In an Address given to the Society of Chemical Industry last year, I indicated another way in which chemical manufacturers can help themselves and at the same time promote the interests of chemistry in this country. In the United States of America individual manufacturers, or associations of manufacturers, have shown themselves ready to take up the scheme originated by the late Professor Duncan for the institution of industrial research scholarships tenable at the universities or technical colleges, and the results obtained after ten years' experience of the working of this practical method of promoting co-operation between science and industry have more than justified the anticipations of its originator. The scheme is worthy of adoption on many grounds, of which the chief are that it provides definite subjects for technical research to young chemists qualified for such work, that it usually leads to positions in factories for chemists who have proved their capacity through the work done while holding scholarships, and that it reacts for good on the profession generally, by bringing about that more intimate intercourse between teachers and manufacturers which is so much to be desired.

In this connection the recent foundation of the Willard Gibbs Chair of research in pure chemistry at the University of Pittsburgh is extremely significant, for it shows that even in such a purely industrial community as Pittsburgh it is recognised that the most pressing need of the day is the endowment of chemical research and the creation of research [illegible] Mr. A. P. Fleming, who recently made a tour of inspection of [illegible] tories in the United States, points to the amount of work done by individual firms and the increased provision now being made for research in universities and technical institutions. He reports that at the present time there are upwards of fifty corporations having research laboratories, costing annually from 20,000*l.* to 100,000*l.* for maintenance, and states that 'some of the most striking features of the research work in America are the lavish manner in which the laboratories have been planned, which in many cases enables large scale operations to be carried out in order to determine the best possible methods of manufacturing any commodity developed or discovered in the laboratories; the increasing attention given in the research laboratories to pure science investigation, this being, in my opinion, the most important phase of industrial research; and the absorption of men who have proven their capacity for industrial research in such places as the Mellon Institute, the Bureau of Standards, &c., by the various industries in which they have taken scientific interest.' It is evidently the view of American manufacturers that industrial research can be made to pay for itself, and that to equip and maintain research laboratories is an excellent investment.

It cannot be too often reiterated that no branch of chemical industry can afford to stand still, for there is no finality in manufacturing processes; all are capable of improvement, and for this, as well as for the discovery and the application of new processes, the services of the trained chemist are essential. Hence the training of chemists for industrial work is a matter of supreme importance. We may therefore congratulate ourselves that the opportunities for

chemical instruction in this country are immensely greater than they were thirty years ago. The claims of chemistry to a leading position have been recognised by all our universities, even the most ancient, by the provision of teaching staffs, laboratories, and equipment on a fairly adequate if not a lavish scale, and in this respect many of the technical colleges fall not far behind. The evening classes conducted in a large number of technical institutions are hardly fitted to produce fully trained chemists, if only because lack of the necessary time prevents the student from [illegible] practice in the laboratory which cannot be dispensed [illegible] prepared to go through a course of study extending over many years. At the same time these evening classes play a most important part, firstly in disseminating a knowledge of chemistry throughout the country, and secondly in affording instruction of a high order in special branches of applied chemistry Finally, in a large and increasing number of schools a more or less satisfactory introduction to the science is given by well-qualified teachers. With our national habit of self-depreciation we are apt to overlook the steady progress which has been made, but at the same time I do not suggest that there is no room for improvement of our system of training chemists. Progress in every department of industrial chemistry is [illegible] dependent upon research, and therefore a sufficient supply of chemists [illegible] knowledge and experience of the methods of research is vital. This being so, it is an unfortunate thing that so many students are allowed to leave the universities in possession of a science degree but without any experience in investigation. The training of the chemist, so far as that training can be given in a teaching institution, must be regarded as incomplete unless it includes some research work, not, of course, because every student has the mental gifts which characterise the born [illegible] but rather because of the inestimable value of the experience gained [illegible] to leave the beaten track and to place more dependence upon his own initiative and resource. Consequently one rejoices to learn that at the University of Oxford no candidate can now obtain an Honours degree without having produced evidence that he has taken part in original research, and that the General Board of Studies at Cambridge has also made proposals which, if adopted, will have the effect of encouraging [illegible] research work Perhaps it is too much to expect that practice in research will be made an indispensable qualification for the ordinary degree; failing this, and indeed in every case, promising students should be encouraged, by the award of research scholarships, to continue their studies for a period of at least two years after taking the B.Sc. degree, and to devote that time to research work which would qualify for a higher degree. In this connection an excelle[illegible] is at hand, for the output of research work from the Scottish [illegible] very greatly increased since the scheme of the Carnegie Trust for the institution of research scholarships has come into operation. Thanks to these scholarships, numbers of capable young graduates, who otherwise for the most part would have had to seek paid employment as soon as their degree courses were completed, have been enabled to devote two or more years to research work. Of course it must be [illegible] at not every chemist has the capacity to initiate or inspire investigat[illegible] no amount of training, however thorough and comprehensive, will make a man an investigator unless he has the natural gift. At the same time, whilst only the few are able to originate really valuable research work, a large army of disciplined men who have had training in the methods of research is required to carry out experimentally the ideas of the master mind. Moreover, there is ample scope in industrial work for chemists who, although not gifted with initiative as investigators, are suitably equipped to supervise and control the running of large-scale processes, the designing of appropriate plant, the working out on the [illegible] scale of new [illegible] improvement of existing ones—men of a thoroughly practical mind, who never lose sight of costs, output, and efficiency, and who have a sufficient knowledge of engineering to make their ideas and suggestions clear to the engineering expert. Further, there has to be considered the necessity for the work of the skilled analyst in the examination of raw materials and the testing of intermediate and finished products, although much of the routine work of the industrial laboratory will advisedly be left in the hands of apprentices working under the control of the chemist. Lastly, for

the buying and selling of materials there should be a demand for the chemist with the commercial faculty highly developed. There is, indeed, in any large industrial establishment room for chemists of several different types, but all of these should have had the best possible training, and it must be the business of our higher teaching institutions to see that this training is provided.

On more than one occasion I have expressed the opinion that every chemist who looks forward to an industrial post should receive in the course of his training a certain amount of instruction in chemical engineering by means of lectures and also of practical work in laboratories fitted for the purpose. The practicability of this has been proved in more than one [illegible] institution, and experience has convinced me that chemists who have [illegible] a course are generally more valuable in a works—whether their ultimate destination is the industrial research laboratory or the control of [illegible] operations—than those who have not had their studies directed [illegible] boundaries of pure chemistry. (I used the word 'traditional' because to my mind there is no boundary line between the domains of pure and of applied chemistry.) A course in chemical engineering, preferably preceded by a short course in general engineering and drawing, must, however, be introduced as *a supplement to*, and not as *a substitute for*, any part of the necessary work in pure chemistry, and consequently the period of undergraduate study will be lengthened if such a course is included; this is n[illegible] but quite the contrary. I am glad to say that the University of Glasgow has recently instituted a degree in Applied Chemistry, for which the curriculum includes chemical engineering in addition to the usual courses in chemistry, and I hope that a plac[illegible] for this subject by other universities.

On the whole, there is not much fault to be found with the training for chemists supplied by the universities and technical colleges, but there is still room for improvements which could and would be carried out if it were not that the scientific departments of these institutions are as a rule hampered by lack of funds. The facilities for practical instruction with respect to accommodation and equipment are general[illegible], on the other hand, the *personnel* could with advantage be [illegible] and at least the junior members of the staffs are miserably underpaid. It would doubtless be regarded as insanity to suggest that a scientific man, however eminent, should receive more than a fraction of the salary to which a music-hall 'artiste' or a lawyer politician can aspire; but if the best brains in the country are to be attracted towards science, as they ought to be, some greater inducement than a mere living wage should be held out. Hence no opportunity should be lost of impressing upon the Government the necessity for increasing the grants to the scientific departments of our higher teaching institutions, and for the provision of research scholarships. It is much to be desired also that wealthy men in this country should take an example from America and acquire more generally the habit of devoting some part of their means to the endowment of higher education. The private donations for science and education made in the United States during the last forty-three years amount to the magnificent sum of 117,000,000*l*., and recently the average annual benefactions for educational purposes total nearly 6,000,000*l*. Of course there are few, if any, of the universities and colleges in this country which are not deeply indebted to the foresight and generosity of private benefactors, but the lavish scale on which funds are provided in America leads to a certain feeling of admiring envy.

After all, the chief difficulty which confronts those who are eager for progress in educational matters is that so many of our most famous schools are still conducted on mediæval lines, in the sense that the 'education' administered is almost wholly classical. Consequently, 'though science enters into every part of modern life, and scientific method is necessary for success in all undertakings, the affairs of the country are in the hands of legislators who not only have little or no acquaintance with the fundamental facts and principles signified by these aspects of knowledge, but also do not understand how such matters can be used to strengthen and develop the State. Our administrative officials are also mostly under the same disabilities, on account of their want of a scientific training. They are educated at schools where science can receive little encouragement, and they do not take up scientific subjects in the examina-

tions for the Civil Service, because marks can be much more easily obtained by attention to Latin and Greek; and the result of it all is that science is usually treated with indifference, often with contempt, and rarely with intelligent appreciation by the statesmen and members of the public services whose decisions and acts largely determine the country's welfare. The defects of a system which places the chief power of an organisation which needs understanding of science in every department in the hands of people who have not received any training in scientific subjects or methods are obvious.'[1] The remedy is also obvious.

Here, again, the prospects are now brighter than ever before, because the warnings and appeals of men of science have at last, and after many years, begun to bear fruit, or perhaps it would be more correct to say the lessons of the war have begun to make an impression on the powers that be. Within the last few weeks it has been intimated that the Government, giving ear to what has been uttered, incessantly and almost *ad nauseam*, with regard to British neglect of science, propose to appoint a committee to inquire into the position of science in our national system of education, especially in universities and secondary schools. The duty of the committee will be to advise the authorities how to promote the advancement of pure science, and also the interests of trade, industries, and professions dependent on the application of science, bearing in mind the needs of what is described as a liberal education. It is stated that the committee will include scientific men in whom the country will have confidence, some of those who appreciate the application of science to commerce and industry, and some who are able from general experience to correlate scientific [illegible] education as a whole. I am sure that we may look forward [illegible] to the recommendations of such a committee, and we shall hope, for the sake of our country, that their recommendations will be adopted and put in force with the least possible delay.

[1] 'Nature,' Feb. 10, 1916.

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ADDRESS

TO THE

CHEMICAL SECTION

BY

C. T. HEYCOCK, M.A., F.R.S.,

PRESIDENT OF THE SECTION.

During its past eighty-nine years of useful life the British Association has, in the course of its evolution, established certain traditions; among these is the expectation that the sectional President shall deliver an address containing a summary of that branch of natural knowledge with which he has become especially acquainted.

The rapid accumulation of experimental observations during the last century, and the consequent necessity for classifying the observed facts with the aid of hypotheses and theories of ever-increasing complexity, make such summaries of knowledge essential, not only to the student of science, but also to the person of non-specialised education who desires to realise something of the tendencies and of the results of modern science.

At the present moment, when the whole world is in pause after having overcome the greatest peril which has ever threatened civilisation; when all productive effort, social, artistic, and scientific, is undergoing reorganisation preparatory to an advance which will eclipse in importance the progress made during the nineteenth century, such attempts to visualise the present condition of knowledge as are made in our Presidential Addresses are of particular value. It is, therefore, hardly necessary for me to apologise for an endeavour to place before you a statement upon the particular branch of science to which I have myself paid special attention; whatever faults may attend the mode of presentation, such a survey of a specific field of knowledge cannot but be of value to some amongst us.

I propose to deal to-day with the manner in which our present rather detailed knowledge of metallic alloys has been acquired, starting from the sparse information which was available thirty or forty years ago;

to show the pitfalls which have been avoided in the theoretical interpretation of the observed facts, and to sketch very briefly the present position of our knowledge.

The production of metals and their alloys undoubtedly constitutes the oldest of those chemical arts which ultimately expanded into the modern science of chemistry, with all its overwhelming mass of experimental detail and its intricate interweaving of theoretical interpretation of the observed facts. Tubal-Cain lived during the lifetime of our common ancestor, and was 'an instructor of every artificer in brass and iron'; and although it may be doubted whether the philologists have yet satisfactorily determined whether Tubal-Cain was really acquainted with the manufacture of such a complex metallic alloy as brass, it is certain that chemical science had its beginnings in the reduction of metals from their ores and in the preparation of useful alloys from those metals In fact, metallic alloys, or mixtures of metals, have been used by mankind for the manufacture of implements of war and of agriculture, of coinage, statuary, cooking vessels, and the like from the very earliest times.

In the course of past ages an immense amount of practical information has been accumulated concerning methods of reducing metals, or mixtures of metals, from their ores, and by subsequent treatment, usually by heating and cooling, of adapting the resulting metallic product to the purpose for which it was required. Until quite recent times, however, the whole of this knowledge was entirely empirical in character, because it had no foundation in general theoretical principles; it was collected in haphazard fashion in accordance with that method of trial and error which led our forerunners surely, but with excessive expenditure of time and effort, to valuable results.

To-day I purpose dealing chiefly with the non-ferrous alloys, not because any essential difference in type exists between the ferrous and non-ferrous alloys, but merely because the whole field presented by the chemistry of the metals and their alloys is too vast to be covered in any reasonable length of time.

The earliest recorded scientific investigations on alloys were made in 1722 by Reaumur, who employed the microscope to examine the fractured surfaces of white and grey cast iron and steel.

In 1808 Widmanstatten cut sections from meteorites, which he polished and etched.

The founder, however, of modern metallography is undoubtedly H. C. Sorby, of Sheffield. Sorby's early petrographic work on the examination of thin sections of rock under the microscope led him to a study of meteorites and of iron and steel, and in a paper read before the British Association in 1864 he describes briefly (I quote his own words) how sections 'of iron and steel may be prepared for the microscope so as to exhibit their structure to a perfection that leaves little to be desired. They show various mixtures of iron, and two or three well-defined compounds of iron and carbon, graphite, and slag; these constituents being present in different proportions and arranged in various manners, give rise to a large number of varieties of iron and steel, differing by well-marked and very striking peculiarities

of structure.' The methods described by Sorby for polishing and etching alloys and his method of vertical illumination (afterwards improved by Beck) are employed to-day by all who work at this branch of metallography.

The lantern-slides, now shown, were reproduced from his original photographs; they form a lasting memorial to his skill as an investigator and his ability as a manipulator. In 1887 Dr. Sorby published a paper on the microscopical structure of iron and steel in the Journal of the Iron and Steel Institute. This masterpiece of clear writing and expression, even with our present knowledge, needs but little emendation. In this paper he describes Free Iron (ferrite) carbon as graphite, the pearly constituent as a very fine laminar structure (pearlitic structure), combined iron as the chief constituent of white cast iron (cementite), slag inclusions, effect of hardening steel, effect of working iron and steel, cementation of wrought iron, and the decarbonisation of cast iron by haematite. A truly remarkable achievement for one man.

From 1854-68 Mattheisen published in the Reports of the British Association and in the Proceedings and Transactions of the Royal Society, a large number of papers on the electrical conductivity, tenacity, and specific gravity of pure metals and alloys. He concluded that alloys are either mixtures of definite chemical compounds with an excess of one or other metal, or solutions of the definite alloy in the excess of one of the metals employed, forming, in their solid condition, what he called a solidified solution. This idea of a solidified solution has developed into a most fruitful theory upon which much of our modern notions of alloys depends. Although, at the time, the experiments on the electrical conductivity did not lead to very definite conclusions, the method has since been used with great success in testing for the presence of minute quantities of impurities in the copper used for conductors.

In the *Philosophical Magazine* for 1875, F. Guthrie, in a remarkable paper, quite unconnected with alloys, gave an account of his experiments on salt solutions and attached water. He was led to undertake this work by a consideration of a paper by Dr. J. Rea, the Arctic explorer, on the comparative saltness of freshly formed and of older ice floes. Guthrie showed that the freezing-point of solutions was continuously diminished as the percentage of common salt increased, and that this lowering increased up to 23.6 per cent. of salt, when the solution solidified as a whole at about 22° C. He further showed, and this is of great importance, that the substance which first separated from solutions more dilute than 23.6 was pure ice. To the substance which froze as a whole, giving crystals of the same composition as the mother liquor, he gave the name cryohydrate. At the time he thought that the cryohydrate of salt containing 23.6 per cent. NaCl and 76.4 per cent. of water was a chemical compound $2NaCl.21H_2O$. In succeeding years he showed that a large number of other salts gave solutions which behaved in a similar manner to common salt. He abandoned the idea that the cryohydrates were chemical compounds.

How clear his views were will be seen by quotations from his

paper in the *Phil. Mag.* (5) I. and II., 1876, in which he states: (i.) When a solution weaker than the cryohydrate loses heat, ice is formed. (ii.) Ice continues to form and the temperature to fall until the cryohydrate is reached. (iii.) At the point of saturation ice and salt separate simultaneously and the solid and liquid portions are identical in composition.

These results can be expressed in the form of a simple diagram as shown in the slide.

In a subsequent paper, *Phil. Mag.* (5) 17, he extends his experiments to solvents other than water, and states that the substances which separate at the lowest temperature are neither atomic nor molecular; this lowest melting-point mixture of two bodies he names the eutectic mixture. In the same paper he details the methods of obtaining various eutectic alloys of bismuth, lead, tin, and cadmium.

We have, in these papers of Guthrie's, the first important clue to what occurs on cooling a fused mixture of metals. The researches of Sorby and Guthrie, undertaken as they were for the sake of investigating natural phenomena, are a remarkable example of how purely scientific experiment can lead to most important practical results. It is not too much to claim for these investigators the honour of being the originators of all our modern ideas of metallurgy. Although much valuable information had been accumulated, no rapid advance could be made until some general theory of solution had been developed. In 1878 Raoult first began his work on the depression of the freezing-point of solvents due to the addition of dissolved substances, and he continued, at frequent intervals, to publish the results of his experiments up to the time of his death in 1901. He established for organic solvents certain general laws: (i.) that for moderate concentrations the fall of the freezing-point is proportional to the weight of the dissolved substance present in a constant weight of solvent; (ii.) that when the falls produced in the same solvent by different dissolved substances are compared, it is found that a molecular weight of a dissolved substance produces the same fall of the [illegible]. whatever the substance is. When, however, he applied the general laws which he had established for organic solvents to aqueous solutions of inorganic acids, bases, and salts, the results obtained were hopelessly discrepant. In a paper in the *Zeit. Physikal. Chem. for* 1888 on 'Osmotic Pressure in the analogy between solutions and gases,' Van't Hoff showed that the experiments of Pfeffer on osmotic pressure could be explained on the theory that dissolved substances were, at any rate for dilute solutions, in a condition similar to that of a gas; that they obeyed the laws of Boyle, Charles, and Avogadro, and that on this assumption the depression of the freezing-point of a solvent could be calculated by means of a simple formula. He also showed that the exceptions which occurred to Raoult's laws, when applied to aqueous solutions of electrolytes, could be explained by the assumption, first made by Arrhenius, that these latter in solution are partly dissociated into their ions. The result of all this work was to establish a general theory applicable to all solutions which has been widespread in its applications. It is true that Van't Hoff's theory has been violently attacked;

but it enables us to calculate the depression of the freezing-points of a large number of solvents. To do this it is necessary to know the latent heat of fusion of the pure solvent and the absolute temperature of the freezing-point of the solution. That the numbers calculated are in very close accord with the experimental values constitutes a strong argument in favour of the theory. From this time the study of alloys began to make rapid progress. Laurie (*Chem. Soc. Jour.* 1888), by measuring the potential difference of voltaic cells composed of plates of alloy and the more negative element immersed in a solution of a salt of one of the component metals, obtained evidence of the existence of compounds such as $CuZn_3.Cu_3Sn$. In 1889 F. H. Neville and I, whilst repeating Raoult's experiments on the lowering of the freezing-point of organic solvents, thought that it was possible that the well-known fact that alloys often freeze at a lower temperature than either of their constituents might be explained in a similar way. In a preliminary note communicated to the Chemical Society on March 21, 1889, on the same evening that Professor Ramsay read his paper on the molecular weights of metals as determined by the depression of the vapour pressure, we showed that the fall produced in the freezing-point of tin by dissolving metals in it was for dilute solutions directly proportional to the concentration. We also showed that the fall produced in the freezing-point of tin by the solution of one atomic weight of metal in 100 atomic weights of tin was a constant.

G. Tannman about the same time (*Zeit. Physikal. Chemie*, III., 44, 1889) arrived at a similar conclusion, using mercury as a solvent.

These experiments helped to establish the similarity between the behaviour of metallic solutions or alloys and that of aqueous and other solutions of organic compounds in organic solvents. That our experiments were correct seemed probable from the agreement between the observed depression of the freezing-point and the value calculated from Van't Hoff's formula for the case of those few metals whose latent heats of fusion had been determined with any approach to accuracy.

Our experiments, subsequently extended to other solvents, led to the conclusion that in the case of most metals dissolved in tin the molecular weight is identical with the atomic weight; in other words, that the metals in solution are monatomic. This conclusion, however, involves certain assumptions. Prof. Ramsay's experiments on the lowering of the vapour pressure of certain amalgams point to a similar conclusion.

So far our work had been carried out with mercury thermometers, standardised against a platinum resistance pyrometer, but it was evident that, if it was to be continued, we must have some method of extending our experiments to alloys which freeze at high temperatures. The thermo couple was not at this stage a reliable instrument; fortunately, however, Callendar and Griffiths had brought to great perfection the electrical resistance pyrometer (*Phil. Trans.* A, 1887 and 1891.) Dr E. H. Griffiths kindly came to our aid, and with his help we installed a complete electrical resistance set. As at this time the freezing-points of pure substances above 300° were not known with any degree of accuracy, we began by making these measurements:—

Table of Freezing-points.

—	Carnelly's Tables	Holborn & Wien, 1892	Callendar & Griffiths, 1892	Neville & H[illegible] [illegible]	Burgess & Le Chatelier, 1912. [illegible] Temperature Measurements
Tin	—	—	231·7	231·9	231·9
Zinc	433	—	417·6	419·0	419·4
Lead	—	—	—	327·6	327·4
Antimony	432	—	—	629·5	630·7 & 629·2
Magnesium	—	—	—	[1]632·6	650
Aluminium	700	—	—	[2]654·5	658
Silver	954	968	972	960·7	960·9
Gold	1,045	1,072	1,037	1,061·7	1,062·4
Copper	1,054	1,082	—	1,080·5	1,083
Sulphur B.P.	448	—	444·53	—	444·7

[1] Contaminated with silicon. [2] Known to be impure.

With the exception of silver and gold, these metals were the purest obtainable in commerce.

Two facts are evident from the consideration of this table: (*a*) the remarkable accuracy of Callendar's formula connecting the Temperature Centigrade with the change of resistance of a pure platinum wire; (*b*) the accuracy of Callendar and Griffith's determination of the boiling-point of sulphur. Although the platinum resistance pyrometer had at this time only been compared with the air thermometer up to 600° C., it will be noted that the exterpolation from 600° to nearly 1100 was justified.

I cannot leave the subject of [illegible] measurements without referring to the specially valuable work of Burgess, and also to Eza Griffiths' book on high-temperature measurements, which contains an excellent summary of the present state of our knowledge of this important subject.

During the period that the above work on non-ferrous alloys was being done, great progress was being made in the study of iron and steel by Osmond and Le Chatelier. In 1890 the Institute of Mechanical Engineers, not apparently without considerable misgivings on the part of some of its [illegible] an Alloys Rese[illegible]. This Committee invited Professor (afterwards Sir William) Roberts-Austen to undertake research work for them. The results of his investigations are contained in a series of five valuable Reports extending from 1891 to 1899, published in the Journal of the Institute. The first report contained a description of an improved form of the Le Chatelier recording pyrometer, and the instrument has since proved a powerful weapon of research. In the second report, issued in 1893, the effects on the properties of copper of small quantities of arsenic, bismuth, and antimony were discussed. Whilst some engineers advocated, others as strongly controverted, the beneficial results of small quantities of

arsenic on the copper used for the fireboxes of locomotives. The report showed that the presence of from ·5-1 per cent. of arsenic was highly beneficial. The third report dealt with electric welding and the production of alloys of iron and aluminium. The fourth report is particularly valuable, as it contains a *résumé* of the Bakerian Lecture given by Roberts-Austen on the diffusion of metals in the solid state, in which he showed that gold, even at as low a temperature as 100°, could penetrate into lead, and that iron became carbonised at a low red heat by contact with a diamond in a vacuum. In 1899 the fifth report appeared on the effects of the addition of carbon to iron. This report is of especial importance, because, besides a description of the thermal effects produced by carbon, which he carefully plotted and photographed, he described the microscopical appearance of the various constituents of iron. The materials of this report, together with the work of Osmond and others on steel and iron, provided much of the material on which Professor Bakhuis Roozeboom founded the iron carbon equilibrium diagram. Reference should also be made to the very valuable paper by Stansfield on the present position of the solution theory of carbonised iron (*Journ. Iron and Steel Inst.*, 11, 1900, p. 317). It may be said of this fifth report, and the two papers just referred to, that they form the most important contribution to the study of iron and steel that has ever been published. Although the diagram for the equilibrium of iron and carbon does not represent the whole of the facts, it affords the most important clue to these alloys, and undoubtedly forms the basis of most of the modern practice of steel manufacture. (Slide showing iron carbon diagram.)

Many workers, both at home and abroad, were now actively engaged in metallurgical work—Stead, Osmond, Le Chatelier, Arnold, Hadfield, Carpenter, Ewing, Rosenhain, and others too numerous to mention.

In 1897 Neville and I determined the complete freezing-point curve of the copper-tin alloys, confirming and extending the work of Roberts-Austen, Stansfield, and Le Chatelier; but the real meaning of the curve remained as much of a mystery as ever. Early in 1900 Sir G. Stokes suggested to us that we should make a microscopic examination of a few bronzes as an aid to the interpretation of the singularities of the freezing-point curve. An account of this work, which occupied us for more than two years, was published as the Bakerian Lecture of the Royal Society in February [illegible]. In preparing a number of copper-tin alloys of known composition we were struck by the fact that the crystalline pattern which developed on the free surface of the slowly cooled alloys was entirely unlike the structure developed by polishing and etching sections cut from the interior; it therefore appeared probable that changes were going on within the alloys as they cooled. In the hope that, as Sorby had shown in the case of steel, we could stereotype or fix the change by sudden cooling, we melted small ingots of the copper-tin alloys and slowly cooled them to selected temperatures and then suddenly chilled them in water. The results of this treatment were communicated to the Royal Society and published in the Proceedings, February 1901. (Slides showing effects of chilling alloys.)

To apply this method to a selected alloy we first determined its cooling curve by means of an automatic recorder, the curve usually showing several halts or steps in it. The temperature of the highest of these steps corresponded with a point on the liquidus, *i.e.*, when solid first separated out from the molten mass. To ascertain what occurred at the subsequent halts, ingots of the melted alloy were slowly cooled to within a few degrees above and below the halt and then chilled, with the result just seen on the screen.

The method of chilling also enabled us to fix, with some degree of accuracy, the position of points on the solidus. If an alloy, chilled when it is partly solid and partly liquid, is polished and etched, it will be seen to consist of large primary combs embedded in a matrix consisting of mother liquor, in which are disseminated numerous small combs, which we called 'chilled primary.' By repeating the process at successively lower and lower temperatures we obtained a point at which the chilled primary no longer formed, *i.e.*, the upper limit of the solidus.

Although we made but few determinations of the physical properties of the alloys, it is needless to say how much they vary with the temperature and with the rapidity with which they are heated or cooled.

From a consideration of the singularities in the liquidus curve, coupled with the microscopic examination of slowly cooled and chilled alloys, we were able to divide the copper-tin alloys into certain groups having special qualities. It would take far too long to discuss these divisions. In interpreting our result we were greatly assisted not only by the application of the phase rule, but also by the application of Roozeboom's theory of solid solution (unfortunately Professor Roozeboom's letters were destroyed by fire in June 1910) and by the advice he kindly gave us. At the time the paper was published we expressly stated that we did not regard all our results as final, as much more work was required to clear up points still obscure. Other workers—Shepherd and Blough, Giolitti and Tavanti—have somewhat modified the diagram. (Slides shown.)

Neither Shepherd and Blough nor Hoyt have published the photomicrographs upon which their results are based, so that it is impossible to criticise their conclusions. Giolitti and Tavanti have published some microphotographs, from which it seems that they had not allowed sufficient time for equilibrium to be established. In this connection I must call attention to the excellent work of Haughton on the constitution of the alloys of copper and tin (*Journ. Institute of Metals*, March 1915). He investigated the alloys rich in tin, and illustrated his conclusions by singularly beautiful microphotographs, and has done much to clear up doubtful points in this region of the diagram. I have dwelt at some length on this work, for copper-tin is probably the first of the binary alloys on which an attempt had been made to determine the changes which take place in passing from one pure constituent to the other. I would again call attention to the fact that without a working theory of solution the interpretation of the results would have been impossible.

Since 1900, many complete equilibrium diagrams have been pub-

lished; amongst them may be mentioned the work of Rosenhain and Tucker on the lead-tin alloys (*Phil. Trans.*, 1908), in which they describe hitherto unsuspected changes on the lead rich side which go on when these alloys are at quite low temperatures, also the constitution of the alloys of aluminium and zinc; the work of Rosenhain and Archbutt (*Phil. Trans.*, 1911), and quite recently the excellent work of Vivian, on the alloys of tin and phosphorus, which has thrown an entirely new light on this difficult subject.

So far I have called attention to some of the difficulties encountered in the examination of binary alloys. When we come to ternary alloys the difficulties of carrying out an investigation are enormously increased, whilst with quaternary alloys they seem almost insurmountable; in the case of steels containing always six, and usually more, constituents, we can only hope to get information by purely empirical methods.

Large numbers of the elements and their compounds which originally were laboriously prepared and investigated in the laboratory and remained dormant as chemical curiosities for many years have, in the fulness of time, taken their places as important and, indeed, essential articles of commerce. Passing over the difficulties encountered by Davy in the preparation of metallic sodium and by Faraday in the production of benzene (both of which materials are manufactured in enormous quantities at the present time), I may remark that even during my own lifetime I have seen a vast number of substances transferred from the category of rare laboratory products to that which comprises materials of the utmost importance to the modern metallurgical industries. A few decades ago, aluminium, chromium, cerium, thorium, tungsten, manganese, magnesium, molybdenum, nickel, calcium and calcium carbide, carborundum, and acetylene, were unknown outside the chemical laboratory of the purely scientific investigator; to-day these elements, their compounds and alloys, are amongst the most valuable of our industrial metallic products. They are essential in the manufacture of high-speed steels, of armour plate, of filaments for the electric bulb lamp, of incandescent gas mantles, and of countless other products of modern scientific industry.

All these metallic elements and compounds were discovered, and their industrial uses foreshadowed, during the course of the purely academic research work carried out in our Universities and Colleges; all have become the materials upon which great and lucrative industries have been built up. Although the scientific worker has certainly not exhibited any cupidity in the past although he has been content to rejoice in his own contributions to knowledge, and to see great manufacturing enterprises founded upon his work it is clear that the obligation devolves upon those who have reaped in the world's markets the fruit of scientific discovery to provide from their harvest the financial aid without which scientific research cannot be continued.

The truth of this statement is well understood by those of our great industrial leaders who are engaged in translating the results of scientific research into technical practice. As evidence of this I may quote the magnificent donation of 210,000*l*. by the British Oil Companies towards the endowment of the school of Chemistry in the University of Cam-

[illegible], the noble bequest of the late Dr. Messel, one of the most en-[illegible] of our technical chemists, for [illegible] the cost of scientific research, the gifts of the late Dr. Ludwig Mond towards the upkeep and expansion of the Royal Institution, one of the strongholds of British chemical research, and the financial support given by the Goldsmiths' and others of the great City of London Livery Companies (initiated largely by the late Sir Frederick Abel, Sir Frederick Bramwell, and Mr. George Matthey), to the foundation of the Imperial College of Science and Technology. The men who initiated these gifts have been themselves intimately associated with developments both in science and industry; they have understood that the field must be prepared before the crop can be reaped. [illegible] our great chemical industries are, for the most part, [illegible] administered by men fully conversant with the mode in which technical progress and prosperity follow upon scientific achievement; and it is my pleasant duty to record that within the last few weeks the directors of one of our greatest [illegible] concerns have, with the consent of their shareholders, devoted £100,000 to research. Doubtless other chemical industries will in due course realise what they have to gain by an adequate appreciation of pure science.

If the effort now being made to establish a comprehensive scheme for the resuscitation of chemical industry within our Empire is to succeed, financial support on a very liberal scale must be forthcoming, from the industry itself, for the advancement of purely scientific research. This question has been treated recently in so able a fashion by Lord Moulton that nothing now remains but to await the results of his appeal for funds in aid of the advancement of pure science.

In order to prevent disappointment, and a possible reaction in the future, in those who endow pure research, it is necessary to give a word of warning. It must be remembered that the history of science abounds in illustrations of discoveries, regarded at the time as trivial which have in after years become epoch-making.

In illustration I would cite Faraday's discovery of electro-magnetic induction. He found that when a bar magnet was thrust into the core of a bobbin of insulated copper wire, whose terminals were connected with [illegible] a momentary current was produced; whilst on withdrawing the magnet a momentary reverse current occurred; a purely scientific experiment destined in later years to develop into the dynamo and with it the whole electrical industry. Another illustration may be given: Guyton de Morveau, Northmore, Davy, Faraday and Cagniard Latour between 1800 and 1850 were engaged in liquefying many of the gases. Hydrogen, oxygen, nitrogen, marsh gas, carbon-monoxide, and nitric oxide, however, resisted all efforts, until the work of Joule and Andrews gave the clue to the causes of failure. [illegible] years later by careful application of the theoretical [illegible] all the gases were liquefied. The liquefaction of oxygen and nitrogen now forms the basis of a very large and important industry.

Such cases can be multiplied indefinitely in all branches of science.

Perhaps the most pressing need of the present day lies in the cultivation of a better [illegible] between our great masters of productive industry, the shareholders to whom they are in the first degree responsible, and our scientific workers; if, by reason of any turbidity of vision, our large [illegible] corporations fail to discern that, in their own interest, the financial support of purely scientific research should be one of their first cares, technical advance will slacken and other nations, adopting a more far-sighted policy, will forge ahead in science and technology. It should, I venture to think, be the bounden duty of everyone who has at heart the aims and objects of the British Association to preach the doctrine that in closer sympathy between all classes of productive labour, manual and intellectual, lies our only hope for the future I cannot do better than conclude by quoting the words of Pope, one of our most characteristically British poets:

> ' By mutual confidence and mutual aid
> Great deeds are done and great discoveries made.'

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SECTION K: NEWCASTLE-ON-TYNE, 1916.

ADDRESS

TO THE

BOTANICAL SECTION

BY

A. B. RENDLE, M.A., D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

Since our last meeting the Great War has continued to hold chief place in our lives and thoughts, and in various ways, and to a greater or less degree, has influenced our work. In the case of many Botany has had for the time being to be set aside, while others have been able to devote only a part of their time to scientific work. On the other hand, it is gratifying to note that some have been able to render helpful service on lines more or less directly connected with their own science. The trained botanist has shown that he may be an eminently adaptable person, capable, after short preparation on special lines, of taking up positions involving scientific investigation of the highest importance from the standpoints of medicine and hygiene.

We have to regret the loss of a promising young Cambridge botanist, Alfred Stanley Marsh, who has made the supreme sacrifice for his country. Happily, in other cases lives have been spared and we are able to welcome their return to the service of botany.

In common with our fellow-botanists throughout the world, we have learnt with sorrow of the death of one of the kindliest and most versatile exponents of the science, Count Solms Laubach, whom we have welcomed in years past as a guest of our Section.

May I also refer to the recognition recently given by the Royal Society to the services of two of our Colonial botanists? Mr. W. H. Maiden, of Sydney, who has done so much in Australia for the development of botany and its applications in his position as Government Botanist and Director of the Botanic Gardens at Sydney, and whose kindness some of us have good cause to remember on the occasion of the visit of this Association to Sydney in 1914; and Professor W. H. Pearson, of Cape Town, who is doing useful work of botanical exploration in South-West Africa.

A little more than two years ago, during the enforced but pleasant leisure of our passage across the Indian Ocean to Australia, I was discussing with our President for the year the possibility of a war with Germany. He was confident that sooner or later it was bound to come. I was doubtful 'But what will prevent it?' asked my companion. 'The common sense of the majority,' was my reply. He was right and I was wrong, but I think he was only less surprised than myself when next evening we heard, by wireless, rumours of the outbreak of what rapidly developed into the great European war. But even a few weeks later, when Germany was pressing westwards, and the very existence of our Empire was threatened, we hardly began to appreciate what it would mean, and we still talked of the possibility of an International Botanical Congress in 1915.

We know more now, and I need not apologise for considering in my Address the part which botanists can take in the near future, especially after the war. For one thing at least is certain, we are two years nearer the end than when it began, and let us see to it that we are not as backward in preparing for post-war as we were for war problems.

Some months ago the various Sectional Committees received a request to

consider what could be done in their respective Sections to meet problems which would arise after the war. Your Committee met and discussed the matter, with the result that a set of queries was sent round to representative botanists asking that suggestions might be presented for consideration by the Committee. A number of suggestions were received of a very varied kind, indicating that in the opinion of many botanists at any rate much might be done to utilise our science and its trained workers in the interests of the State and Empire. Your Committee decided to arrange for reports to be prepared on several of the more important aspects by members who were specially fitted to discuss these aspects, and these will be presented in the course of the meeting. These reports will, I am convinced, be of great value, and may lead to helpful discussion; they may also open up the way to useful work.

For my own part, while I might have preferred to consider in my Address some subject of more purely botanical interest, I felt that under the circumstances an academic discourse would be out of place, and that I too must endeavour to do something to effect a more cordial understanding between botany and its economic applications.

For many of us this means the breaking of new ground. We have taken up the science because we loved it, and if we have been able to shed any light on its numerous problems the work has brought its own reward. But some of us have on occasion been brought into touch with economic problems, and such must have felt how inadequate was our national equipment for dealing with some of these. In recent years we have made several beginnings but these beginnings must expand mightily if present and future needs are to be adequately [illegible] we are determined to make the best use of the material to our hand.

Whether or no we have been living for the past forty years in a fools' paradise, it is certain that our outlook will be widely different after the war, and may the stimulus of a changed environment find us ready to respond!

Sacrifice must be general, and the botanist must do his bit. This need not mean giving up the pursuit of pure science, but it should mean a heavy specialisation in those lines of pure science which will help to alleviate the common burden, will render our country and the Empire less dependent on external aid, and knit more closely its component parts.

It may be convenient to consider, so far as they are separable, Home and Imperial problems.

Without trenching on the domain of Economics, we may assume that increased production of foodstuffs, timber, and other economic products will be desirable. [illegible] has been raised as to the possibility of increasing at the same time industrial and agricultural development. But as in industry perfection of machinery allows a greater output with a diminished number of hands, so in agriculture and horticulture perfection of the machinery of organisation and equipment will have the same result.

There are three factors in which botanists are primarily interested—the plant, the soil, and the worker.

The improvement of the plant from an economic point of view implies the co-operation of the botanist and the [illegible] The student of experimental genetics, by directing his work t[illegible]ic value, is able, with the help of the resources of agricultur[illegible], to produce forms of greater economic value, kinds best suited to different localities and ranges of climate, those most immune to disease and of the highest food-value. Let the practical man formulate the ideal, and then let the scientist be invited to supply it. Much valuable work has been done on these lines, but there is still plenty of scope for the organised Mendelian study of plants of economic importance. It is a very large subject, and we are hoping to hear more about it before we separate.

A minor example occurs to me. Do the prize vegetables which one sees at shows and portrayed in the catalogues represent the best products from an economic point of view; in other words, is the standard of excellence one which considers solely their value as foodstuffs? A chemico-botanical examination would determine at what point increase in size becomes disproportionate to increase in food-value, and thus correct the standard from an economic point of view. And, presumably, the various characters which imply greater or less feeding value offer scope for the work of the Mendelian.

The subject of intensive cultivation offers a series of problems which are primarily botanical. It would be a useful piece of investigation to work out the most profitable series which can be grown from year to year with the least expenditure on manures and the minimum of liability to disease. A comparatively small area would suffice for the work.

The introduction of new plants of economic value is within the range of possibility; our repertoire has increased in recent years, but an exhaustive study of food plants and possible food plants for man and stock would doubtless yield good results. It is matter of history that the introduction of the tea plant into further India was the result of observations by Fortune, a botanical collector. The scientific botanist may find pleasant relaxation in the smaller problems of horticulture.

We have heard much lately as to the growing of medicinal plants, and experience would indicate that here is opportunity for investigation, and, unless due care is taken, also danger of waste of time, money, and effort. A careful systematic study of species, varieties, and races is in some cases desirable in order to ensure the growth of the most productive or valuable plant, as in the case of the Aconites; and such a study might also reveal useful substitutes or additions. Here the co-operation between the scientific worker and the commercial man is imperative. I have recently been interested to hear that the special properties of medicinal plants are to be subjected to experiment on Mendelian lines.

During the past year there has been considerable activity in the collecting of [illegible] of various species of medicinal value, frequently, one fears, [illegible] of time and waste of plants, owing to want of botanical or technical knowledge and lack of organisation. In this connection a useful piece of botanical work has recently been carried out by Mr. W. W. Smith, of Edinburgh, on the collection of sphagnum for the preparation of surgical dressings. The areas within the Edinburgh district have been mapped and classified so as to indicate their respective values in terms of yield of sphagnum. By the indication of the most suitable areas, the suitability depending on extent of area, density of growth, freedom of admixture of grass or heather, as well as facility of transport and provision of labour, the report is of great economic value. The continuity of supply is an important question, and one which should be borne in mind by collectors of medicinal plants generally. And while it is not the most favourable time to voice the claims of protection of wild plants, one may express the hope that the collector's zeal will be accompanied by discretion.

The advantages arising from a closer co-operation between the practical man and the botanist is illustrated by [illegible] laboratories recently organised by the Royal Horticultural Society at Wisley. Such an institution forms a common meeting-ground for [illegible] of plants and the botanist. The former sets the problems, and the latter takes them in hand under conditions approaching the ideal and with the advantages of mutual discussion and critic[illegible] such as these will give ample [illegible] the enthusiastic young botanist who is anxious to embark on w[illegible]. The student of plant physiology will find here work of great interest. The grower has perforce gained a great deal of information as to the behaviour of his plants under more or less artificial conditions, but he is unable to analyse these conditions, and the [illegible] of the physiologist is an invaluable help. Experiments in the [illegible] ants under the influence of [illegible] electricity are at the present time being carried out at Wisley. Such experiments may be conducted anywhere where land and power are available, but it is obviously advantageous that they should be conducted by an expert plant-physiologist versed in scientific method and not directly interested in the result. Dr. Keeble's recent series of lectures on Modern Horticulture at the Royal Institution deal with matter which is full of interest to the botanist. For instance, he shows how the work of Continental botanists on the forcing of plants has indicated methods, in some cases simple and inexpensive, which have proved of considerable commercial value, and that there is evidently scope for work in this direction, which, while of interest to the plant-physiologist, may be also of general utility.

The subject of the soil offers problems to the botanist as well as to the

chemist and proto-zoologist. In the plant we are dealing with a living organism, not a machine; and an adequate knowledge of the organism is essential to a proper study of its [illegible] growth. The facility with which a considerable sum of money was raised just before the war to improve the equipment at Rothamsted, where work was being done on these lines, indicates that practical men are ready to come forward with financial help if work which promises to yield results of economic importance is being seriously carried out. And it is significant of the attitude of botanists to such problems that there is only one trained botanist on the staff of this institution.

The study of manures and their effect on the plant should attract the botanist as well as the chemist. In this connection I may refer to Mr. Martin Sutton's recent work at R[illegible] effects of radio-active ores and residues on plant-life. A series [illegible] was carried out in two successive years with various subject[illegible] the different character of their produce, and including roots, tubers, bulbs, foliage, and fruit. From the immediate point of view of agriculture and horticulture the results were negative; the experiments gave no hope of the successful employment of radium as an aid to either the farmer or gardener. Speaking generally, the produce from a given area was less when the soil had been treated with pure radium bromide, or various proprietary radio-active fertilisers, than when treated with farmyard manure or a complete fertiliser; while the cost of dressing was very much greater. To quote Mr. Sutton's concluding words, 'The door is still open to the investigator in search of a plant fertiliser which will prove superior to farmyard dung or the many excellent artificial preparations now available.' But though the immediate result was unsatisfactory to the grower, there were several points of interest which would have appealed to the botanist who was watching the course of the experiments, and which, if followed up, might throw light on the effect of radium on plant-life and lead in the end to some useful result. As Mr. Sutton points out, many of the results were 'contradictory,' while a close examination of the trial notes, together with the records of weights, will furnish highly interesting problems. For instance, there was evidence in some cases that germination was accelerated by presence of radium, though subsequent growth was retarded; and the fact that in several of the experiments plants dressed with a complete fertiliser in addition to radium have not done so well as those dressed with the fertiliser only may be regarded as corroborating M. Truffaut's suggestion that radium might possess the power of releasing additional nitrogen in the soil for the use of plants, and that the plants in question were suffering from an excess of nitrogen. Certain remarkable variations between the duplicate unmanured control plots in several of the experiments led to the suggestion that radium emanations may have some effect, apparently a beneficial one. I have quoted these experiments as an example of a case where the co-operation of the botanist and the practical man might lead to useful results, and at the same time afford work of much interest to the botanist.

As an introduction to such work University Professors might encourage their advanced students to spend their long vacation in a large nursery or botanic garden where experimental work is done.

As regards the worker in agriculture and horticulture, how can the botanist help? Apart from well-staffed and well-equipped schools of agriculture and horticulture, which require the botanist's assistance, a wider dissemination of the botanist would be advantageous. Properly trained botanists distributed through the country with their eyes open might be a valuable asset in the improvement of production; botanist and cultivator might be mutually helpful; the former would meet problems at first hand, and the latter should be encouraged by the co-operation. A kind of first-aid class suggests itself, run by a teacher with a good elementary knowledge of botany upon which has been erected a [illegible] knowledge of horticultural [illegible]. This would afford a vocation for students of scientific bent who cannot spare the time for a long University course. Some of us may remember the courses arranged by various County Councils thirty years or so ago, financed by the whiskey money, out of which have grown some useful permanent educational institutions. But these courses were often barren of result, owing partly to insufficient 'sympathy' between the lecturer and his audience. A young man fresh from the University who was waiting for a more permanent job was brought into touch with the practical man in the lecture hall, and the contact was, so to speak, not good.

Between the two was a gulf across which the lecturer shouted, and his words often conveyed little meaning to those on the other side. A great deal of money must have been spent with incommensurate results.

On the other hand, we must be careful to work economically and not wear out high-class tools on rough work. I think there is some danger of this in connection with certain courses in horticulture for women. Girls who have had a good general education enter, at the age of seventeen or eighteen, on a course of study, lasting for two or three years, of horticultural methods and the kindred sciences. So far, good; but after all this training the finished product should aspire to something more than market gardening in competition with the man who left school at twelve or fourteen, has learnt his business practically, and has a much lower standard of living.

The utilisation of waste lands is a big subject and trenches on the domain of Economics. But important botanical problems are involved and careful ecological study will prepare the way for serious experimental work. The study of the growth of plants in alien situations is fraught with so many surprises and apparent contradictions that successful results may be looked for in most unlikely situations. I remember a striking instance near Lake Tarawera, in the North Island of New Zealand. The area in question had been completely devastated in the great eruption of Mount Tarawera in 1886, th[illegible] being covered with ash to a depth of several feet. When I saw it two years ago the vegetation of a considerable area was almost purely Central European. The trees were poplar, *Robinia*, and elder, with an undergrowth of dog-rose, bramble, &c. I was not able to find out the recent history of the locality and there were very few signs of habitation, but it was not the kind of vegetation one would expect to find growing so naturally and freely in such a locality. But the subject of utilisation of waste lands will occupy us later.

The study of the diseases to which plants are liable, and their prevention and cure, offers a wide and increasing field for inquiry, and demands a larger supply of trained workers and a more definite and special system of training. For the study of those which are due to fungi it is obviously essential that a thorough general knowledge of fungi and laboratory methods should be acquired, preferably at some Pathological Institution which would also be in touch with the cultivator and naturally approached by those requiring advice and help in connection with disease, on the same principle that a medical school is attached to a hospital. An important part of the training should be the study of the disease in the field and the conditions under which it arises and flourishes. From the point of view of Mycology much useful scientific work remains to be done on the life history of the fungi which are or may be the causes of disease. The study of preventive methods must obviously be carried out in the field, and, while these are mainly mechanical processes, they need careful supervision; the question of the subsequent gathering and disposal of a crop must not be overlooked. Experiments in the use of dust instead of spray as a preventive of fungous and insect attack have recently been carried out in America. Other plant diseases afford problems for the physiologist, who is a necessary part of the equipment of the Pathological Institute.

The anatomical and chemical study of timbers might with advantage occupy a greater number of workers. The matter is of great economic importance. Questions of identity are continually arising, and in the present vague state of our knowledge it is often difficult or impossible to give a satisfactory answer. Samples of timber are put on the market shipped, say, from West Africa under some general name such as [illegible] the importer does not supply leaves and flowers f[illegible] purpose of [illegible] and in the present incomplete state of our knowledge it is often impossible to make more than a vague attempt at determination. Or a merchant brings a sample which has been sent from X as Y, which it obviously is not; but what is it, whence does it probably come, and what supply of it is likely to be forthcoming? These are questions which it would be useful to be able to answer with some [illegible]oach to accuracy than at present. And it should be the work [illegible] trained persons. I recall a sample of wood which some months ago, coming from a Government Department, went the round of the various institutions which were at all likely to be able to

supply the required information as to its identity. It should have been matter of common knowledge where to apply, with at the same time reasonable certainty of obtaining the information required.

It is possible also that a more systematic study of minute structure would help t[illegible] of affinity. A chemical study has proved of value in the [illegible] the species of *Eucalyptus* in Australia.

Apart from co-operation between the botanist and the practical or commercial man, there is need for co-ordination between workers. I give the following incident from real life. At the meeting of an advisory committee the head of a certain institution stated that he had set one of his staff to work at a certain disease which was then under discussion, but had learnt shortly after that a student at another institution was engaged on the same piece of work. A conference led to a useful division, one of the workers to study the life history of the organism in the laboratory, the other to work at conditions of life, &c., in the field. But it also transpired that another institution, as well as another independent worker, were engaged on the same problem, and while it was suggested that in one case co-operation might be invited, it was deemed inadvisable to approach the other. The problem in this case was not one of such special difficulty as to require so much attention, and even if it had been some co-ordination between the various working units would have been helpful. Similar instances will occur to you. The measure of efficiency of our science should be the sum of the efficiency of its workers. It should be possible to devise some means for informing fellow-workers as to the piece of work in hand or proposed to be undertaken, and thus on the one hand to avoid wasteful expenditure of time and effort, and not infrequently the [illegible] incomplete results, and on the other to ensure where [illegible] of co-operation.

The various illustrative suggestions which I have made would imply a close co-operation between the schools of botany and colleges and institutions of agriculture, horticulture, and forestry; to pass from the former to one or other of the latter for special work or training should be a natural thing. While on the one hand a University course is not an essential preliminary to the study of one or other of the applied branches, the advantages of a broad, general training in the principles of the science cannot be gainsaid. The establishment of professorships, readerships, or lectureships in economic botany at the University would supply a useful link between the pure and applied sciences, while research fellowships or scholarships would be an incentive t[illegible]

There is the wider question of a rapprochement between the man of science and the commercial man. Its [illegible] and the advantages would be mutual; on the one hand it would secure the spread and application of the results of research, and on the other hand the man of science would be directed to economic problems of which otherwise he might not become cognisant. The closer association between the academic institution and those devoted to the application of the science would be a step in this direction.

Our British possessions, especially within the tropics, contain a wealth of material of economic value which has been only partially explored. One of the first needs is a tabulation of the material. In the important series of Colonial floras incepted by Sir Joseph Hooker, and [illegible] under the [illegible] Kew, lies the foundation for further work. Consider, for instance, the 'Flora of Tropical Africa,' now rapidly nearing completion. This is a careful and, so far as possible with the material at hand, critical descriptive catalogue of the plants from tropical Africa which are preserved in the great British and European Herbaria. The work has been done by men with considerable training in systematic work, but who know nothing at first hand of the country the vegetation of which they are cataloguing. Such a 'Flora' must be regarded as a basis [illegible] further work. Its study will indicate botanical areas and their characteristics, and suggest what areas are likely to prove of greater or less economic value, and on what special lines. It will also indicate the lines on which areas may be mapped out for more detailed botanical exploration. That this is necessary is obvious to any botanist who has used such a work. A large proportion of the species, some of which may, on further investigation, prove to be of economic value, are known only from a single incomplete fragment. Others, for instance, which may be of known economic value, doubtless exist

over much larger areas and in much greater quantity than would appear from the 'Flora.' The reason of these shortcomings is equally obvious. The collections on which the work is based are largely the result of voluntary effort employed more or less spasmodically. The explorer working out some new route, who brings what he can conveniently carry to illustrate the plant products of the new country; the Government official or his wife, working during their brief leisure or collecting on the track between their different stations: the missionary or soldier, with a penchant for natural history; to these and similar persons we are largely indebted for additions to our knowledge of the plant-life. [illegible] has sometimes been taken of a Government expedition to which a [illegible] man with a knowledge of or taste for natural history, or, in rare cases, a trained botanist, has been attached.

The specimens brought home by the amateur collector often leave much to be desired, and little or no information is given as to precise locality or the nature of the locality, the habit of the plant, or other items of importance or interest. There may be indications that the plant is of economic value, but no information as to whether it is rare or plentiful, local or occurring over a wide area.

Samples of wood are often brought, but generally without any means of identification except a native name; and it must be borne in mind that native names are apt to be misleading; they may be invented on the spur of the moment to satisfy the white man's craving for information or when genuine are often applied to more than one species.

A large proportion of the more extensive collections are due to German enterprise, and the best representation of this work is naturally to be found in Germany, though it is only fair to state that the German botanists have been generous in lending material for work or comparison. The botanical investigation of German East Africa and the Cameroons has been carried out by well-trained botanists and collectors, and the results of their work published both from botanical and economic points of view. I may refer to the large volume on German East Africa, which contains not only a general account of the vegetation and a systematic list of the genera and species comprising the flora, but also an account of the plants of economic value classified according to their uses. The exploration of the Belgian Congo has been seriously undertaken by the Belgian Government, and a number of large and extensively illustrated botanical memoirs have been issued. Some of us may be familiar with the fine Congo Museum near Brussels.

It is time that pioneer work gave place to systematic botanical exploration of our tropical possessions and [illegible] of handy working floras and economic handbooks. Work of [illegible] should be full of interest to the young botanist. But if he is to make the best use of time and opportunity he must have had a proper course of training. After [illegible] his general botanical course, which should naturally include an [illegible] principles of classification, he should work for a time in a large Herbarium and thus acquire a knowledge of the details of systematic work and also of the general outlines of the flora of the area which he is to visit later. He should then be given a definite piece of work in the botanical survey of the area. From the collated results of such work convenient handbooks on the botanical resources of regions open to British enterprise could be compiled There will be plenty of work for the systematist who cannot leave home. The ultimate elaboration of the floristic work must be done in the Herbarium with its associated library. There is also need of a careful monographic study of genera of economic value which would be best done by the experienced systematist at home, given a plentiful supply of carefully collected and annotated material. An example of such is the systematic account of the species of *Sansevieria* by Mr. N. E Brown, recently issued at Kew. Closely allied species or varieties of one and the same species may differ greatly in economic value, and the work of the monographer is to discover and diagnose these different forms and elucidate them for the benefit of the worker in the field.

If we are to make the best use of our resources botanical research stations in different parts of the Empire, adequately equipped and under the charge of a capable trained botanist, are a prime necessity. We seem to have been singularly unfortunate, not to say stupid, in the management of some of our tropical stations and botanical establishments.

The island of Jamaica is one of the oldest of our tropical possessions. It is easy of access, has a remarkably rich and varied flora, a fine climate, and affords easy access to positions of widely differing altitude. It is interesting to imagine what Germany would have made of it as a station for botanical work if she had occupied it for a few years. The most recent account of the flora which pretends to completeness is by Hans Sloane, whose work antedates the Linnæan era. A flora as complete as available material will allow is now in course of preparation in this country, but the more recent material on which it is based is due to American effort. Comparatively recently a mycologist has been appointed, but there is no Government botanist to initiate botanical [illegible] or experimental work or to advise on matters of botanical interest. [illegible] station ideal for experimental work in [illegible] problems is a mere appendage of a Department of Agriculture [illegible] of which is a chemist.

A botanical station for research to be effective must be under the supervision of a well-trained botanist with administrative capacity, who must have at his disposal a well-equipped laboratory and ground for experimental work. He must not be expected to make his station pay its way by selling produce or distributing seedlings and the like; a botanical station is not a market-garden. The Director will be ready to give help and advice on questions of a botanical nature arising locally, and he will be on the look-out for local problems which may afford items of botanical research to visiting students. Means must be adopted to attract the research student, aided if necessary by research scholarships from home. The station should have sufficient Imperial support to avoid the hampering of its utility by local prejudice or ignorance. The permanent staff should include a mycologist and a skilled gardener.

The botanical station does not preclude the separate existence of an agricultural station, but the scope of each must be clearly defined, and under normal conditions the two would be mutually helpful. Nor should the botanical station be responsible for work of forestry, though forestry may supply problems of interest and importance for its consideration.

Finally, I should like to suggest the holding of an Imperial Botanical Congress at which matters of general and special interest might be discussed. The visit of the British Association to Australia was, I think, helpful to the Australian botanists; it was certainly very helpful and of the greatest interest to those coming from home. Many of the addresses and papers were of considerable interest and value, but of greater value [illegible] meeting with one's fellow-workers in different fields, of [illegible] and interchange of ideas, the better realisation of one's limited outlook and the stimulus of new associations. A meeting which brought together home botanists and botanical representatives from oversea portions of our Empire to discuss methods of better utilising our vast resources would be of great interest and supremely helpful. Let us transfer to peace purposes some of the magnificent enthusiasm which has flowed homewards for the defence of the Empire in war.

In this brief address I have tried, however imperfectly, to indicate some lines on which botanists may render useful service to the community. To a large extent it means the [illegible] and extension of existing facilities added to an organised [illegible] botanists themselves and between botanists and the practical and commercial man; this will include an efficient, systematic cataloguing of work done and in progress. We do not propose to hand over all our best botanists to the applied branches and to starve pure research, but our aim should be to find a useful career for an increasing number of well-trained botanists and to ensure that our [illegible] Empire shall make the best use of the results of our research. [illegible] there will be an increased demand for the teaching botanist, for he will be responsible for laying the foundations.

Complaint has been made in the past that there were not enough openings for the trained botanist; but if the responsibilities and opportunities of the science are realised we may say, rather, 'Truly the harvest is plentiful, but the labourers are few.' Botany is the *alma mater* of the applied sciences, agriculture, horticulture, forestry, and others; but the *alma mater* who is to receive the due affection and respect of her offspring must realise and live up to her responsibilities.

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British Association for the Advancement of Science.

SECTION K: CARDIFF, 1920.

ADDRESS

TO THE

BOTANICAL SECTION

BY

MISS E. R. SAUNDERS, F.L.S.,

PRESIDENT OF THE SECTION.

YEAR by year we see the meetings of the Association recur, pursuing a course which neither geographer nor astronomer would venture to predict and leaving traced out behind them a figure unknown to the mathematician. Nevertheless the path of its journeyings is ever returning upon itself. As this recurrence is brought afresh to one's mind, there is a natural impulse to reflect upon the progress which has been made in the intervening period in the science which one here finds oneself called upon to represent. Not quite thirty years have elapsed since the last occasion on which the Association was welcomed to Cardiff. Curiosity to learn whether the matter of the discourse delivered by my predecessor on that occasion had a connection, close or remote, with the particular subject with which I proposed to deal in this Address led me to refer to the Annual Report of the Association for 1891. I thus became aware how recent was the occurrence of the mutation—or should I rather say of the dichotomy?—which led to the appearance of a Botanical Section, for twenty-nine years ago Section K had not yet come into existence. At that period the problems relating to living organisms, whether concerned with plant or animal, whether of a morphological or physiological nature, were all embraced within the wide field of Section D, the Section of Biology. Though in succeeding years discovery at an ever-increasing rate and in many new fields of investigation has made inevitable the separation first of Physiology, and then of Botany from their common parent, we may with advantage follow the precedent set by the Association as a whole, and, as a Section, return from time to time upon our course of evolution. I shall therefore invite your attention to a subject which lies within the wide province of Biology and makes its appeal alike to the botanist, zoologist and physiologist—the subject of Heredity.

By the term Inheritance we are accustomed to signify the obvious fact of the resemblance displayed by all living organisms between offspring and parents, as the direct outcome of the contributions received from the two sides of the pedigree at fertilisation: to indicate, in fact,

owing to lack of knowledge of the workings of the hereditary process, merely the *visible* consequence—the final result of a chain of events. Now, however, that we have made a beginning in our analysis of the stages which culminate in the appearance of any character, a certain looseness becomes apparent in our ordinary use of the word Heredity, covering as it does the two concomitant essentials, genetic potentiality and somatic expression—a looseness which may lead us into the paradoxical statement that inheritance is wanting in a case in which nevertheless the evidence shows that the genetic constitution of the children is precisely like that of the parents. When we say that a character is inherited no ambiguity is involved, because the appearance of the character entails the inheritance of the genetic potentiality. But when a character is stated not to be inherited it is not thereby indicated whether this result is due to environmental conditions, to genetic constitution, or to both causes combined. That we are now able in some measure to analyse the genetic potentialities of the individual is due to one of those far-reaching discoveries which change our whole outlook, and bring immediately in their train a rapidly increasing array of new facts, falling at once into line with our new conceptions, or by some orderly and constant discrepancy pointing a fresh direction for attack. An historical survey of the steps by which we have advanced to the present state of our knowledge of Heredity has so frequently been given during the last twenty years that the briefest reference to this part of my subject will suffice.

The earliest attempts to frame some general law which would co-ordinate and explain the observed facts of inheritance were those of Galton and Pearson. Galton's observations led him to formulate two principles which he believed to be capable of general application—the Law of Ancestral Heredity and the Law of Regression. The Law of Ancestral Heredity was intended to furnish a general expression for the sum of the heritage handed on in any generation to the succeeding offspring. Superposed upon the working of this law were the effects of the Law of Regression, in which the average deviation from the mean of a whole population of any fraternal group within that population was expressed in terms of the average deviation of the parents. These expressions represent statements of averages which, in so far as they apply, hold only when large numbers are totalled together. They afford no means of certain prediction in the individual case. These and all similar statistical statements of the effects of inheritance take no account of the essentially physiological nature of this as of all other processes in the living organism. They leave us unenlightened on the fundamental question of the nature of the means by which the results we witness came to pass. We obtain from them, as from the melting-pot, various new products whose properties are of interest from other viewpoints, but, corresponding to no biological reality, they have failed to bring us nearer to our goal—a fuller comprehension of the workings of the hereditary mechanism. Progress in this direction has resulted from the opposite method of inquiry—the study of a single character in a single line of descent, the method which deals with the unit in place of the mass. The revelation came with the opening of the present

century, for in 1900 was announced the rediscovery of Mendel's work actually given to the world thirty-five years earlier, but at the time leaving no impress upon scientific thought. The story of the Austrian monk and the details of his experiments carried out in the monastery garden upon races of the edible pea are now familiar history, and I need not recount them here. Having formed the idea that in order to arrive at a clearer understanding of the relation of organisms to their progeny the problem must be studied in its simplest form, Mendel came to see that a scheme of analysis must deal not with mass populations but with a smaller unit—the family, and that each character of the individual must be separately [illegible].

Selecting for his experiments races which showed themselves to be pure-breeding and mating together those exhibiting characters of such opposite nature as to constitute a pair—*e.g.*, tall with short, yellow-seeded with green-seeded—he obtained results which could be accounted for if it were supposed that these opposite, or as we should now term them allelomorphic, characters were distributed *unaltered and in equal proportion* to the reproductive cells of the cross-bred organism. It is this conception of the pure nature of the germ-cells, irrespective of whether the organism forming them be of pure-bred or cross-bred descent, which revolutionised our conceptions of Heredity and laid the foundations upon which we build to-day. For the intervening years have seen the instances in which the Mendelian theory is found to hold mount steadily from day to day, furnishing a weight of evidence in its support which is incontrovertible.

It chanced that in each pair of characters selected by Mendel for experiment the opposites are related to each other in the following simple manner: An individual which had received both allelomorphs, one from either parent, exhibited one of the two characteristics, hence called the dominant, to the exclusion of the other. Among the offspring of such an individual both characteristics appeared, the dominant in some, its opposite, the recessive, in others, in the proportion approximately of three to one. This is the result which might be expected from random pairing in fertilisation of two opposites, where the manifestation in the zygote of the one completely masks the presence of the other. As workers along Mendelian lines increased and the field of inquiry widened, it soon, however, became apparent that the dominant-recessive relationship is not of universal occurrence. It likewise became clear that the simple ratios which obtained in Mendel's experiments are not characteristic of every case. Mendel's own results were all, as it happened, explicable on the supposition that the two alternative forms of each character were dependent on a *single* element or factor. By a fortunate accident none of the complex factorial inter-relations which have since been brought to light in other cases obscured the expression in its simplest form of the results of germ purity. It is our task, in the light of this [illegible] principle, to attempt to elucidate these more complicated types [illegible].

We now know, for example, that many characters are not controlled by one single factor, but by two or more. One of the most familiar instances of the two-factor character is the appearance of the

colouring matter anthocyanin in the petals of plants such as the Stock and Sweet Pea. Our proof that two factors (at least) are here involved is obtained when we find that two true breeding forms devoid of colour yield coloured offspring when mated together. In this case the two complementary factors are carried, one by each of the two crossed forms. When both factors meet in the one individual, colour is developed. We have in such cases the solution of the familiar, but previously unexplained, phenomenon of Reversion. Confirmatory evidence is afforded when among the offspring of such cross-bred individuals we find the simple 3 to 1 ratio of the one-factor difference replaced by a ratio of 9 to 7. Similarly we deduce from a ratio of 27 to 37 that three factors are concerned, from a ratio of 81 to 175 four factors, and so on. The occurrence of these higher ratios proves that the hereditary process follows the same course whatever the number of factors controlling the character in question.

And here I may pause to dwell for a moment upon a point of which it is well that we should remind ourselves from time to time, since, though tacitly recognised, it finds no explicit expression in our ordinary representation of genetic relations. The method of factorial analysis based on the results of inter-breeding enables us to ascertain the least possible number of genetic factors concerned in controlling a particular somatic character, but what the total of such factors actually is we cannot tell, since our only criterion is the number by which the forms we employ are found to differ. How many may be common to these forms remains unknown. In illustration I may take the case of surface character in the genera *Lychnis* and *Matthiola*. In *L. vespertina* the type form is hairy; in the variety *glabra*, recessive to the type, hairs are entirely lacking. Here all glabrous individuals have so far proved to be similar in constitution, and when bred with the type give a 3 to 1 ratio in F_2.[1] We speak of hairiness in this case, therefore, as being a one-factor character. In the case of *Matthiola incana v. glabra*, of which many strains are in cultivation, it so happened that the commercial material originally employed in these investigations contained all the factors since identified as present in the type and essential to the manifestation of hairiness except one. Hence it appeared at first that here also hairiness must be controlled, as in *Lychnis*, by a single factor. But further experiment revealed the fact that though the total number of factors contained in these glabrous forms was the same, the respective factorial combinations were not identical. By inter-breeding these and other strains obtained later, hairy F_1 cross-breds were produced giving ratios in F_2 which proved that at least four distinct factors are concerned.[2] Whereas, then, the glabrous appearance in *Lychnis* always indicates the loss (if for convenience we may so represent the nature of the recessive condition) *of one and the same factor*, analysis in the Stock shows that the glabrous condition results if *any factor out of a group of four* is represented by its recessive allelomorph. Hence we describe hairiness in the latter case as a four-factor character.

[1] *Report to the Evolution Committee, Royal Society*, i., 1902.
[2] *Proc. Roy. Soc.* B, vol. 85, 1912.

It will be apparent from the cases cited that we cannot infer from the genetic analysis of one type that the factorial relations involved are the same for the corresponding character in another. That this should be so in wholly unrelated plants is not perhaps surprising, but we find it to be true also where the nature of the characteristic and the relationship of the types might have led us to expect uniformity. This is well seen in the case of a morphological feature distinctive of the N.O. Gramineæ. The leaf is normally ligulate, but individuals are occasionally met with in which the ligule is wanting. In these plants, as a consequence, the leaf blade stands nearly erect instead of spreading out horizontally. Nilsson-Ehle [3] discovered that in Oats there are at least four and possibly five distinct factors determining ligule formation, all with equal potentialities in this direction. Hence, only when the complete series is lacking is the ligule wanting. In mixed families the proportion of ligulate to non-ligulate [illegible] depends upon the number of these ligule-producing factors contained in the dominant parent. Emerson [4] found, on the other hand, that in Maize mixed families showed constantly a 3 to 1 ratio, indicating the existence of only one controlling factor.

From time to time the objection has been raised that the Mendelian type of inheritance is not exhibited in the case of specific characters. That no such sharp line of distinction can be drawn between the behaviour of varietal and specific features has been repeatedly demonstrated. As a case in point and one of the earliest in which clear proof of Mendelian segregation was obtained, we may instance *Datura*. The two forms, *D. Stramonium* and *D. Tatula*, are ranked by all systematists as distinct species. Among other specific differences is the flower colour. The one form has purple flowers, the other pure white. In the case of both species a variety *inermis* is known in which the prickles characteristic of the fruit in the type are wanting. It has been found that in whatever way the two pairs of opposite characters are combined in a cross between the species, the F_2 generation is mixed, comprising the four possible combinations in the proportions which we should expect in the case of two independently inherited pairs of characters, when each pair of opposites shows the dominant-recessive relation. Segregation is as sharp and clean in the specific character flower colour as in the varietal character of the fruit. Among the latest additions to the list of specific hybrids showing Mendelian inheritance, those occurring in the genus *Salix* are of special interest, since heretofore the data available had been interpreted as conflicting with the Mendelian conception. The recent observations of Heribert-Nilson [5] show that those characters which are regarded by systematists as constituting the most distinctive marks of the species are referable to an extremely simple factorial system, and that the factors mendelise in the ordinary way. Furthermore, these specific-character factors

[3] *Kreuzungsuntersuchungen an Hafer und Weizen*, Lund, 1909.

[4] Annual Report of the Agricultural Experiment Station of the University of Nebraska, 1912.

[5] *Experimentelle Studien über Variabilität, Spaltung, Artbildung und Evolution in der Gattung Salix*, 1918.

control not only the large constant *morphological features*, but *fundamental reactions* such as those determining the condition of physiological equilibrium and vitality in general. In so far as any distinction can be drawn between the behaviour of factors determining the varietal as opposed to the specific characters of the systematist, Heribert-Nilsson concludes that the former are more localised in their action, while the latter produce more diffuse results, which may affect almost all the organs and functions of the individual, and thus bring about striking alterations in the general appearance. *S. caprea*, for example, is regarded as the reaction product of two distinct factors which together control the leaf-breadth character, but which also affect, each separately and in a different way, leaf form, leaf colour, height, and the periodicity of certain phases. We cannot, however, draw a hard-and-fast line between the two categories. The factor controlling a varietal characteristic often produces effects in different parts of the plant. For example, the factors which lead to the production of a coloured flower no doubt also in certain cases cause the tinging seen in the vegetative organs, and affect the colour of the seed. Heribert-Nilsson suggests that fertility between species is a matter of close similarity in genotypic (factorial) constitution rather than of outward morphological resemblance. Forms sundered by the systematist on the ground of gross differences in certain anatomical features may prove to be more akin, more compatible in constitution, than others held to be more nearly related because the differentiating factors happen to control less conspicuous features.

Turning to the consideration of the more complex types of inheritance already referred to, we find numerous instances where a somatic character shows a certain degree of coupling or linkage with another perhaps wholly unrelated character. This phenomenon becomes still further complicated when, as is now known to occur fairly frequently, somatic characters are linked also with the sex character. The results of such linkages appear in the altered proportions in which the various combinations of the several characters appear on cross-breeding. Linkage of somatic characters can be readily demonstrated in the garden Stock. Some strains have flowers with deeply coloured sap, *e.g.*, full red or purple; others are of a pale shade such as a light purple or flesh colour. In most commercial strains the 'eye' of the flower is white owing to absence of colour in the plastids, but in some the plastids are cream-coloured, causing the sap colour to appear of a much richer hue and giving a cream 'eye.' Cream plastid colour is recessive to white and the deep sap colours are recessive to the pale. When a cream-eyed strain lacking the pale factor is bred with a white-eyed plant of some pale shade, the four possible combinations appear in F_2 but not, as we should expect in the case of two independently inherited one-factor characters, in the proportions 9 : 3 : 3 : 1, with the double recessive as the least abundant of the four forms. We find instead that the double dominant and the double recessive are both in excess of expectation, the latter being more abundant than either of the combinations of one dominant character with one recessive. The two forms which preponderate are those which exhibit the

combinations seen in the parents, the two smaller categories are those representing the new combinations of one paternal with one maternal characters. In the Sweet Pea several characters are linked in this manner, viz.: flower colour with pollen shape, flower colour with form of standard, pollen shape with form of standard, colour of leaf axil with functioning capacity of the anthers. If in these cases the cross happens to be made in such a way that the two dominant characters are brought in one from each side of the pedigree instead of both being contributed by one parent, we get again a result in which the two parental combinations occur more frequently, the two recombinations or 'crossovers' less often than we should expect. In the first case the two characters appear to hang together in descent to a certain extent but not completely, in the latter similarly to repel each other. This type of relationship has been found to be of very general occurrence. The linked characters do not otherwise appear to be connected in any way that we can trace, and we therefore conclude that the explanation must be sought in the mechanism of distribution. Two main theories having this fundamental principle as their basis but otherwise distinct have been put forward, and are usually referred to as the *reduplication* and the *chromosome* view respectively. The reduplication view, proposed by Bateson and Punnett,[6] rests on the idea that segregation of factors need not necessarily occur simultaneously at a particular cell division. The number of divisions following the segregation of some factors being assumed to be greater than those occurring in the case of others, there would naturally result a larger number of gametes carrying some factorial combinations and fewer carrying others. If this differential process is conceived as occurring in an orderly manner it would enable us to account for the facts observed. We could imagine how it came about that gametic ratios such as 3 : 1 : 1 : 3, 7 : 1 : 1 : 7, 15 : 1 : 1 : 15, and so on arose giving the series of linkages observed. It has, however, to be said that we cannot say *why* segregation should be successive nor at what moments, on this view, it must be presumed to occur. On the other hand, the conceptions embodied in the chromosome hypothesis as formulated by Morgan and his fellow-workers[7] is, in these respects, quite precise. They are built around one cardinal event in the life cycle of animals and plants (some of the lowest forms excepted). viz.: the peculiar type of cell division at which the number of chromosomes is reduced to half that to be found during the period of the life cycle extending backwards from this moment to the previous act of fertilisation. In the large number of cases already investigated the chromosome number has been found as a rule to be the same at each division of the somatic cells. We can, in fact, take it as established that it is ordinarily constant for the species. These observations lend strong support to the view that the chromosomes are persistent structures, that is to say, that the chromatin tangle of the resting nucleus, whether actually composed of one continuous thread or not, becomes resolved into

[6] *Proc. Roy. Soc.*, 1911.

[7] *The Mechanism of Mendelian Heredity* (Morgan, Sturtevant, Muller, Bridges), 1915

separate chromosomes at corresponding loci at each successive mitosis. The reduction from the diploid to the haploid number, according to the more generally accepted interpretation of the appearances during the meiotic phase, is due to the adhering together in pairs of homologous chromosomes, each member of the set originally received from one parent lying alongside and in close contact with its mate received from the other. Later these bivalent chromosomes are resolved into their components so that the two groups destined one for either pole consist of whole dissimilar chromosomes, which then proceed to divide again longitudinally to furnish equivalent half chromosomes to each of the daughter nuclei. According to the view of Farmer the homologous chromosomes do not lie alongside, but become joined end to end. The longitudinal split seen in the bivalent structure is interpreted as a separation not of whole chromosomes but of half chromosomes already formed in anticipation of the second division of the meiotic phase. As however on either interpretation the same result is ultimately secured, viz.: the distribution of whole paternal and maternal chromosomes to different nuclei which now contain the haploid number, it is not essential to our present purpose to discuss the cytological evidence in support of these opposing views in further detail. Nor, indeed, would it be practicable within the limits of this Address. The obvious close parallel between the behaviour of the chromosomes—their pairing and separation—and that of Mendelian allelomorphs which similarly show pairing and segregation, first led to the suggestion that the factors controlling somatic characters are located in these structures. The ingenious extension of this view which has been elaborated by Morgan and his co-workers presumes the arrangement of the factors in linear series after the manner of the visible chromomeres—the beadlike elements which can be seen in many organisms to compose the chromatin structure—each factor and its opposite occupying corresponding loci in homologous chromosomes. From this conception follows the important corollary of the segregation of the factors during the process of formation and subsequent resolution of the bivalent chromosomes formed at the reduction division. We should suppose, according to Morgan, in the case of characters showing *independent* inheritance and giving identical Mendelian ratios whichever way the mating is made, and however the factorial combination is brought about, that the factors controlling the several characters are located in *different* chromosomes. Thus, in the case of *Datura* already mentioned, the two factors affecting sap colour and prickliness respectively would be presumed to be located so far apart in the resting chromatin thread that when separation into chromosomes takes place they become distributed to different members. Where unrelated characters show a *linked* inheritance the factors concerned are held on the other hand to lie so near together that they are always located in one and the same chromosome. Furthermore, and here we come to the most debatable of the assumptions in Morgan's theory, when the bivalent chromosome composed of a maternal and a paternal component gives rise at the reduction division to two single dissimilar chromosomes, these new chromosomes do not always represent the original intact maternal and paternal

components. It has been observed in many forms that the bivalent structure has the appearance of a twisted double thread. Already in 1909 cytological study of the salamander had led Janssen[8] to conclude that fusion might take place at the crossing points, so that when the twin members ultimately draw apart each is composed of alternate portions of the original pair. Morgan explains the breeding results obtained with *Drosophila* by a somewhat similar hypothesis. He also conceives that in the process of separation of the twin lengths of chromatin cleavage between the two is not always clean, portions of the one becoming interchanged with corresponding segments of the other, so that both daughter chromosomes are made up of complementary sections of the maternal and paternal members of the duplex chromosome. To picture this let us imagine that two bars of that delectable substance, Turkish Delight, one pink and one white, are laid alongside and are then given a half twist round each other and pressed together. If, with a knife inserted between the two pieces at one end, the double bar is now sliced longitudinally down the middle neither of the two halves will be wholly pink or wholly white. Each half will be particoloured, the pink portion in one and the portion which is white in the other representing corresponding regions of the original bars. If the complete twist is n [illegible] number of turns is still further increased before the slicing, the number of alternately coloured portions will naturally be increased correspondingly. Though the precise manner in which the postulated chromosomal interchange is brought about in Janssen's 'chiasmatype' and Morgan's 'crossing-over' scheme is different, the resulting gametic output would be the same. A critical examination by Wilson and Morgan,[9] from different aspects, of Janssen's interpretation of the cytological evidence including discussion of his latest suggestion that in the case of compound ring chromosomes cleavage in one plane would result in the separation of homologous elements in one ring but not in another has just appeared. These authors are not disposed to accept Janssen's conclusions,[10] but reserve their final statement pending the appearance of his promised further contribution. Should Janssen's view of the evolutions of these complex chromosome structures be upheld, the process of segregation might in such cases become extended over more than one mitosis, as on the reduplication theory is conceived to be the case at some point, though evidence in this direction has hitherto been lacking. Bisection of a bivalent chromosome in this fashion might, moreover, yield the class of results to explain which Morgan has found it necessary to have recourse to hypothetical lethal factors. On the main issue, however, both schemes are in accord. A physical basis for the phenomenon of linkage is found in the presumed nature and behaviour of the chromosomes, viz.: their colloidal consistency, their adhesion after pairing at the points of contact, when in the twisted condition, and their consequent failure to separate cleanly before undergoing the succeeding division.

[8] *La Cellule*, xxv.
[9] *Am. Nat.*, vol. 54, 1920.
[10] See *Comptes Rendus Soc. Belg. Biol.*, 1919.

According to Morgan the frequency of separation of linked characters is a measure of the distance apart in the chromosome of the loci for the factors concerned, and it becomes possible to map their position in the chromosome relatively to one another. In this attempt to find in cytological happenings a basis for the observed facts of inheritance our conception of the material unit in the sorting-out process has been pushed beyond the germ cell and even the entire chromosome to the component sections and particles of the latter structure.

To substantiate the 'chromosome' view the primary requisite was to obtain proof that a particular character is associated with a particular chromosome. With this object in view it was sought to discover a type in which individual chromosomes could be identified. Several observers working on different animals found that a particular chromosome differing in form from the rest could be traced at the maturation division, and that this chromosome was always associated with the sex character in the following manner. The female possessed an even number of chromosomes so that each egg received an identical number, including this particular sex-chromosome. The male contained an uneven number, having one fewer than the female, with the result that half the sperms received the same number as the egg including the sex-chromosome, and half were deficient in this particular chromosome. Eggs fertilised with sperms containing the full number of chromosomes developed into females, while those fertilised with sperms lacking this distinctive chromosome produced males. Morgan made the further discovery in the fruit fly *Drosophila ampelophila* that certain factors controlling various somatic characters were located in the sex-chromosome. The inheritance of these characters and of sex evidently went together. A male exhibiting the dominant condition of such a sex-linked character bred to a recessive female gave daughters all dominant and sons all recessive (fig. 2), but in the reciprocal cross both sons *and* daughters proved to be all dominants (fig. 1). Since the mother with the dominant factor contributed it to all her children (fig. 1), whereas, where the father bore it, it descended only to his daughters (fig. 2), it was apparent that the female was homozygous and the male heterozygous for the *somatic* character. Further, although no distinction is observable in this species between the sperms, the occurrence of this sex-linked form of inheritance indicated that here, as in the other cases mentioned, it is the female which behaves as a homozygote for the *sex* character and the male as a heterozygote, the sex-chromosomes of some sperms differing presumably in character, though not in appearance, from those of others. The sperms of *Drosophila* are therefore conceived as of two kinds, one containing the same sex-chromosome as the eggs, the so-called X chromosome, and the other a mate of a different nature, the Y chromosome, which appears to be inert and unable to carry the dominant allelomorphs. If, now, we suppose the factor for the sex-linked somatic character to be located in the X chromosomes we understand why the dominant female, which is XX, and therefore furnishes an X chromosome to every egg, should contribute the dominant character to all her

offspring. And conversely, why the dominant male, which is XY, when bred to a recessive female, produces offspring which are either female and dominant or male and recessive.

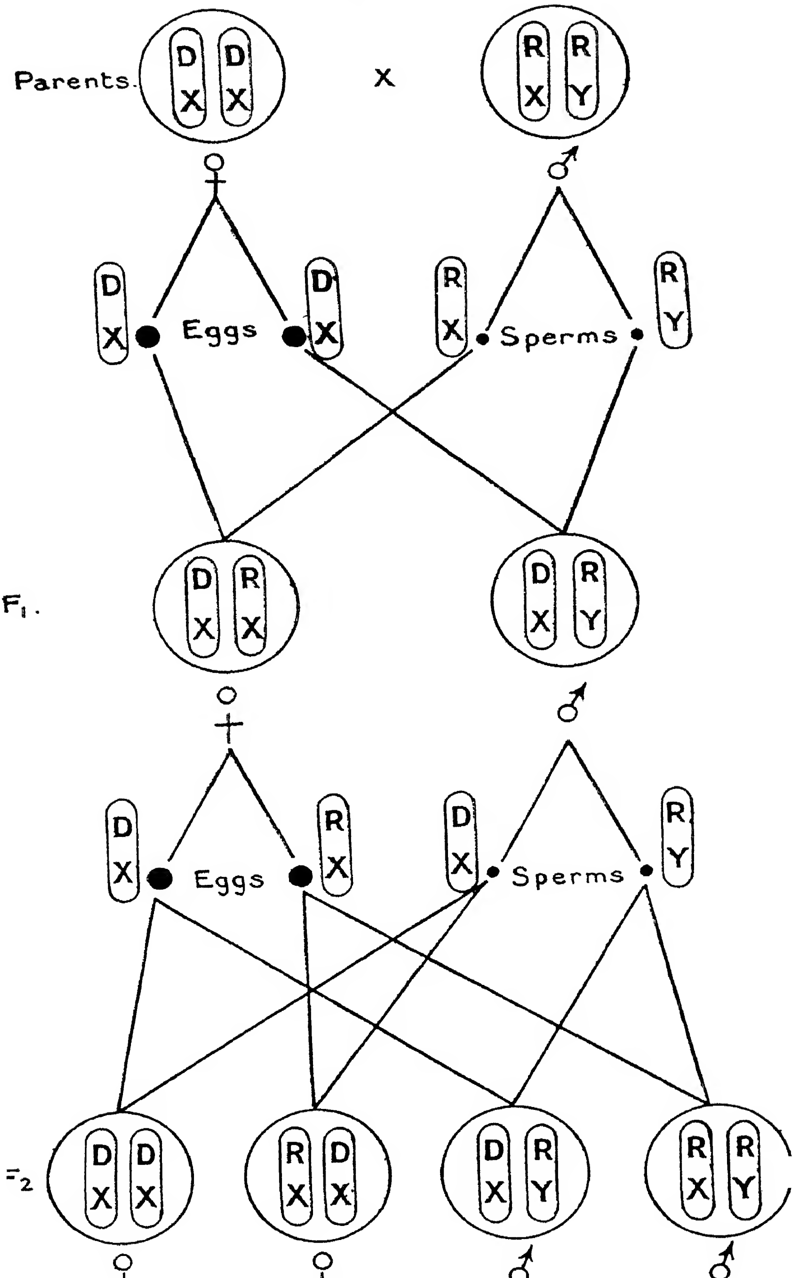
Tracing the chromosomes into the next (F_2) generation we see also the reason for the different result obtained from the reciprocal matings if the F_1 individuals are inbred. When the *female* parent has the dominant sex-linked character half the eggs of the daughters and half the sperms of the sons receive this character. As the sperms receive it along with the X chromosome fertilisation of either kind of egg by these X sperms will cause the character to descend to each grand-daughter. The grandsons, on the other hand, since they arise from fertilisation by the sperms lacking the dominant character—*i.e.*, by the Y sperms—will be dominant or recessive according as these sperms unite with the one type of egg or with the other. Thus we get the Mendelian F_2 ratio 3D to 1R (fig. 1), but so linked with sex that the dominant class comprises half the males and all the females, while the remaining half of the males make up the recessive class. Where it is the *male* parent that carries the dominant, and where therefore the dominant character passes along with the X chromosome *only* to the daughters in F_1, their eggs, as in the reciprocal cross, are of two kinds, but the sons' sperms all carry the recessive allelomorphs. Both kinds of eggs being fertilised with both X sperms and Y sperms, the dominant and recessive characters will occur *equally in both sexes* among the grandchildren, and we get the Mendelian ratio of 1D to 1R (fig. 2). Muller [11] puts the number of factors already located in the X chromosome of *Drosophila* at not less than 500, and in those that have so far been investigated this form of inheritance has been found to hold.

Instances of sex-linked inheritance are now known in many animals, some of which are strictly comparable with *Drosophila*, others follow the same general principle, but have the relations of the sexes reversed, as exemplified by the moth *Abraxas*, which has been worked out by Doncaster,[12] whose sudden death we have so recently to deplore. Here the female is the heterozygous sex, and contains the dummy mate of the sex-chromosome.

The behaviour of the sex-chromosomes as here outlined suffices to account for the occurrence of sex-linked inheritance, but the relations found to hold between one sex-linked character and another need further explanation. If a cross is made involving two sex-linked characters, the F_1 females when tested by a double recessive male are found to produce the expected four classes of gametes, but not in equal proportions, nor in the same proportions in the case of different pairs of sex-linked characters. Partial linkage (coupling) occurs of the kind which has already been described for the Stock and the Sweet Pea. The parental combinations predominate, the recombinations ('cross-overs') comprise the smaller categories. The strength of the linkage varies, however, for different characters, but is found to be constant for any given pair. Since the sex-linked factors are by hypothesis

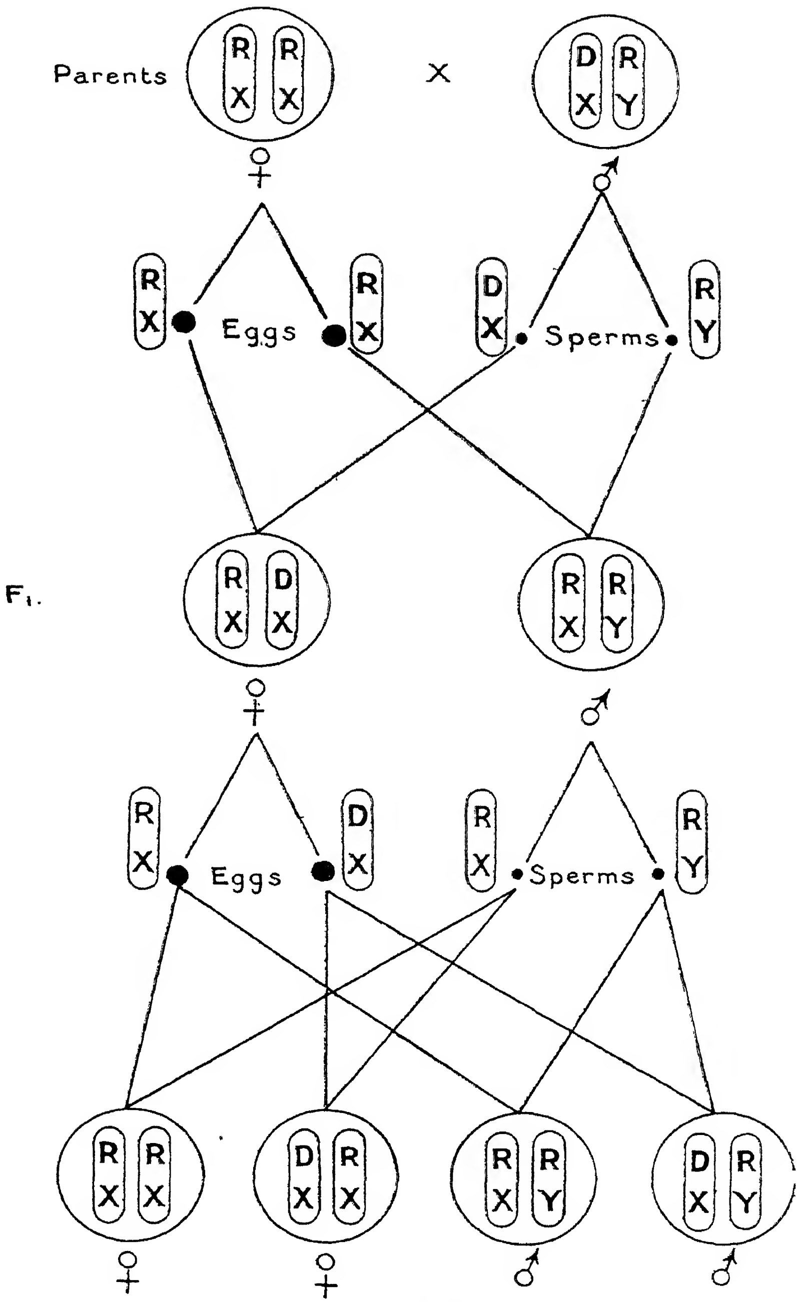
[11] *Am. Nat.*, vol. liv., 1920.
[12] *Rep. Evolution Committee*, iv., 1908.

Fig. I


Parents.
D X
D X
x
R X
R Y
♀
♂
D X
Eggs
D X
R X
Sperms
R Y
F1.
D X
R X
D X
R Y
♀
♂
D X
Eggs
R X
D X
Sperms
R Y
F2
D X
D X
R X
D X
D X
R Y
R X
R Y
♀
♀
♂
♂

3 D : 1 R

Fig. 2


Parents
R R X X
♀
X
D R X Y
♂
R X
Eggs
R X
D X
Sperms
R Y
F₁.
R D X X
♀
R R X Y
♂
R X
Eggs
D X
R X
Sperms
R Y
R R X X
♀
D R X X
♀
R R X Y
♂
D R X Y
♂

1 D 1 R.

carried in the sex-chromosomes, a clean separation of homologous members at meiosis should result in the characters which were associated in the parents remaining strictly in the same combination in each succeeding generation. The fact that this is not the case has led Morgan to conclude that an interchange of chromosome material must take place at this phase among a proportion of the gametes, and that the percentage of these 'cross-overs' will depend on the distance apart of [illegible] the factors concerned. This phenomenon of linkage may also be exhibited by pairs of characters which show *no* sex-linkage in their inheritance. The factors involved in these latter cases must presumably, therefore, be disposed in one of the chromosomes which is not the sex-chromosome.

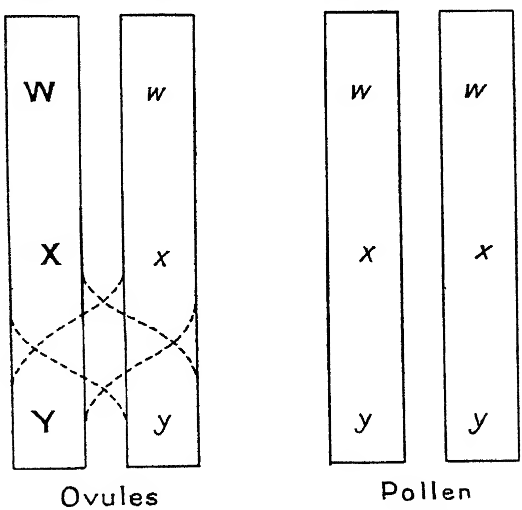
To this brief sketch of the main points of Morgan's chromosome theory must be added mention of the extremely interesting relation which lends strong support to his view, and the significance of which seems scarcely to admit of question, viz.: that in *Drosophila ampelophila* there are four pairs of chromosomes, and that the linkage relations of the hundred and more characters investigated indicate that they form four distinct groups. It is hardly possible to suppose that the one fact is not directly connected with the other. The interesting discovery of Bridges [13] that the appearance of certain unexpected categories among *Drosophila* offspring, where females of a particular strain were used, coincided with the presence in these females of an additional chromosome adds another link in the chain of evidence. On examination it was found that in these females the X chromosome pair occasionally failed to separate at the reduction division, and consequently that the two XX chromosomes sometimes both remained in the egg, and sometimes both passed out into the polar body. Hence there arose from fertilisation of the XX eggs some individuals containing *three* sex-chromosomes, with the resulting upset of the expectation in regard to sex-limitation of characters which was observed.

It, however, remains a curious anomaly that in the cross-bred *Drosophila* male no corresponding crossing-over of linked characters, whether associated with the sex character or not, has yet been observed. His gametes carry only the same factorial combinations which he received from his parents. For this contrast in the behaviour between the sexes there is at present no explanation. The reverse condition has been described by Tanaka[14] in the silkworm. Here interchange takes place in the male but not in the female.

It must then be acknowledged that Morgan's interpretation of the cytological evidence has much in its favour. The striking parallel between the behaviour of the chromosomes and the [illegible] relations of Mendelian allelomorphs is obvious. The existence in *Drosophila ampelophila* of four pairs of chromosomes and of four sets of linked characters can hardly be mere coincidence. The employment of the smaller physical unit in accounting for the reshuffling of characters in their transmission commends itself in principle. The necessity for postulating the occurrence of some orderly irregularity in the hereditary

[13] *J. Exp. Zool.*, xv., 1913.
[14] *J. Coll. Agr.*, Sapporo, Japan, 1913-14.

process in order to explain the phenomenon of partial linkage is, it will be seen, inherent alike in both theories. When, however, we come to examine the general applicability of Morgan's theory we are confronted with a considerable body of facts among plants which we find difficult to reconcile with the requirement that factorial segregation is accomplished by means of the reduction division. An instance in which this is particularly clearly indicated is that of the sulphur-white Stock. I have chosen this example because here we have to do with two characters which are distinguished with the utmost sharpness, viz.: plastid colour and flower form. The peculiar behaviour of this strain is due to the fact that not only are the two factors for flower form (singleness and doubleness) differently distributed to the male and female sides of the individual, as in all double-throwing Stocks, but the factor controlling plastid colour likewise shows linkage with the sex nature of the germ cells. As a result every individual, even though self-fertilised, yields a mixed offspring, consisting chiefly of single whites and double creams, but including a small percentage of double whites. So far as the ovules are concerned, the mode of inheritance can be accounted for on either theory. According to the reduplication hypothesis the factors X Y [15] producing singleness and W giving white plastids are partially coupled so as to give the gametic ratio on the female side 7 WXY : 1 WXy : 1 wxY : 7 wxy.[16] On the chromosome scheme the factorial group WXY must be assumed to be disposed in one member of the bivalent chromosome formed at meiosis, the corresponding recessive allelomorphs wxy in the other. If the three factors be supposed to be arranged in the chromosome in alphabetical order, and if, on separation, a break takes place between the loci of the two factors for flower form (as shown), so as to give 'cross-overs' of Y



[15] The letters X and Y are used here to denote particular factors, not, as in Morgan's scheme, the entire sex-chromosomes.
[16] Or possibly 15 : 1 : 1 : 15.

and y in about 12 per cent. of the gametes, the occurrence of such 'cross-overs' would fulfil the required conditions. But the case of the pollen presents a distinct difficulty on this latter view. This Stock is distinguished both from the *Drosophila* and the *Abraxas* type by the fact that none of the male germs carry either of the dominant characters. In place of the XX—XY form of sex-linked inheritance in the former type and the WZ—ZZ in the latter, we should need to regard this form as constituting a new class, which we might represent as DR—RR, thus [illegible] both members of the bivalent chromosome on the male side appear to be inert and able to carry only the recessive characters, and hence are represented as RR, in contrast with the DR pair of the female side. By this formula we can indicate the behaviour of the several double-throwing strains. It is, besides, becoming clear, I think, from recent results that there is no 'crossing over' of these factors on the male side in the F_1 cross-breds. But the real difficulty is to explain why these factors are confined to the female side in the ever-sporting individual. This may result from aberrant behaviour or loss of chromosomes at some point in pollen development. On this point I hope that evidence will shortly be available. Failing such evidence the presumption is that the elimination of XY (and in one strain of W) must have taken place *prior to*, and not *at*, the moment of the maturation division. Morgan's proposal to fit the pollen into his scheme for *Drosophila* by having recourse to hypothetical lethal factors does not appeal to the observer, who finds the pollen all uniformly good and every ovule set. Zygotic lethals are clearly not in question under these circumstances. The supposition of gametic lethals confined to the pollen appears far-fetched, seeing that of the missing combinations two, viz.: single white, the double dominant, and double white a dominant-recessive, occur in the ovules, and the third, the single cream, the other dominant-recessive, exists as a pure strain, so that the homozygous condition is evidently not in itself a cause of non-development. Other examples suggesting premeiotic segregation can be quoted, notably cases among variegated plants and plants showing bud sports, where somatic segregation appears to be of regular occurrence. Among the Musciniae the present evidence appears to show that the sex potentiality segregates in some forms at the division of the spore mother cells, so that already the spores possess a sex character; while in other species this separation takes place later, during the development of the gametophyte, the spores being then all alike and undifferentiated in this respect. In *Funaria hygrometrica*, an example of the latter class, an attempt has been made by E. J. Collins[17] to ascertain the stage at which sex segregation takes place by inducing the growth of new individuals from isolated portions of the [illegible] tissues of the [illegible]. No doubt when the evidence is derived from experiments in which a portion of the plant has been severed from the rest, it is possible to urge that the result obtained is not necessarily indicative of the potentiality in the intact organism. Phenotypic appearance is the product of a reaction system, in which the internal as well as the external environment plays its part. We

[17] *Journal of Genetics*, vol. viii., 1919.

have, for example, evidence that the manifestation of a character may be dependent upon the variation of internal conditions with age; in other words, a time relation may be involved.[18] Or, again, upon the state of general internal equilibrium resulting from the relation of one morphological member or region to another. Thus removal of the lamina of the leaf, so as to leave only the midrib, may cause the mutilated individual to develop hairs on the stems and petioles in the same environment in which the intact individual remains hairless. Injury from attack by insects in a glabrous form may in like manner lead to the production of hairs which, by their resemblance to those of an allied species, show that the pathological condition set up has caused genetic potentiality to become actual. But even if we exclude the class of evidence to which objection on these grounds might be made, there still remain various cases of normal types, where, unless the behaviour of the chromosomes should point to a different explanation, it seems most natural to assume that segregation takes place before the reduction division.

It has been urged from time to time that any scheme representing the mechanism of heredity which leaves out of account the cytoplasm must prove inadequate. This general statement has been expressed in more definite form by Loeb,[19] who holds that the egg cytoplasm is to be looked upon as determining the broad outlines, in fact as standing for the embryo 'in the rough,' upon which are impressed in the course of development the characteristics controlled by the factors segregated in the chromosomes. The arguments in favour of the view that the cytoplasm, apart from its general functions in connection with growth and nutrition, is the seat of a particular hereditary process are mainly derived from observation upon embryonic characters in certain animals, chiefly Echinoderms, where the inheritance appears to be purely maternal. It has been shown, however, that such female prepotency is no indication that inheritance of the determining factors takes place through the cytoplasm. Other causes may lead to this result. It has been observed, for example, that hybrid sea-urchin larvae, which at one season of the year were maternal in type, at another were all paternal in character, showing that the result was due to some effect of the environment. Again, where the hybrid plutei showed purely maternal characters it was discovered by Baltzer[20] that in the earliest mitoses of the cross-fertilised eggs a certain number of chromosomes fail to reach the poles, and are consequently left out of the daughter nuclei. The chromosomes thus lost probably represent those contributed by the male gamete, for in both parents certain individual chromosomes can be identified owing to differences in shape and size. After this process of elimination those characteristic of the male parent could not be traced, whereas the one pair distinctive of the female parent was still recognisable. In the reciprocal cross where the first mitosis

[18] As in the case of characters which exhibit a regular change of phase, *e.g.*, the colour of white and cream Stocks is indistinguishable in the bud, and a yellow-seeded Pea is green at an earlier stage.

[19] *The Organism as a Whole*, 1916.

[20] *Archiv für Zellforschung*, v., 1910.

follows a normal course the embryos are intermediate in regard to character of the skeleton, thus affording proof of the influence of the male parent. Another type of case is found in the silkworm. Here a certain rate character determining the time of hatching out of the eggs has been shown to exhibit normal Mendelian inheritance, the appearance that it is transmissible by the female through the cytoplasm alone being delusive. The eggs are always laid in the spring. According as they hatch out immediately so that a second brood is obtained in the year, or do not hatch out for twelve months, the female parent laying the eggs is described as bivoltin or univoltin. Now the length of interval before hatching is obviously an egg character, and therefore maternal in origin. Consequently when a cross is made between a univoltin female and a bivoltin male the eggs laid are not cross-bred in respect of this character, any more than the seed formed as a result of a cross is cross-bred in respect of its seed coat, which is a maternal structure. The silkworm mother being univoltin, the eggs will not hatch out until the following spring. The F_1 mother will in turn lay eggs which again take twelve months to hatch, since the long-period factor is the dominant. It is not until the eggs of the F_2 generation are laid that we see the expression of the character introduced by the univoltin father. For some of the egg batches hatch at once, others not for twelve months, showing that of the F_2 females some were uni- and some bi-voltin, and hence that the egg character in any generation depends upon both the maternal and the paternal antecedents of the female producing the eggs. Consequently, in the case of an egg character the effects of inheritance must be looked for in the generation succeeding that in which the somatic characteristics of the zygote become revealed. We find in fact that in almost all instances where the evidence is suggestive of purely cytoplasmic inheritance, fuller investigation has shown that the explanation is to be found in one of the causes here indicated. The case of some plants where it has been established that reciprocal hybrids are dissimilar still, however, remains to be cleared up. Among such may be cited certain Digitalis hybrids. Differences in the reciprocal hybrids of *D. grandiflora* and *D. lutea* were described by Gaertner, and in the earlier literature dealing with Digitalis species hybrids other cases are to be found. In more recent years J. H. Wilson[21] has repeated the crossing of *D. purpurea* and *D. lutea*, and states that the reciprocals are indistinguishable during the vegetative period, but that they differ in size and colouring of the flowers, the resemblance being the greater in each case to the seed parent. A detailed comparison of [illegible] characters of the reciprocal hybrids of *D. purpurea* and *D. grandiflora* has been set out by Neilson Jones,[22] who similarly finds in both matings a greater resemblance to the mother species. We know nothing as yet of the cytology of these cases, and it is not improbable that the interpretation may be found in some aberrant behaviour of the chromosomes. An instance in a plant type where a definite connection appears traceable between chromosome behaviour and somatic appearance has been

[21] *Rep. Third International Congress on Genetics*, R.H.S. 1906.
[22] *J. of Genetics*, vol. ii., 1912.

recently emphasised by Gates,[23] who attributes the peculiarity of the *lata* mutation in Œnothera (which has arisen as a modification at different times from each of three distinct species) to an irregularity in meiosis in the germ mother cells whereby one daughter cell receives an extra duplicate chromosome while the sister cell lacks this chromosome. The cell with the extra chromosome fertilised by a normal germ produces a *lata* individual. On the chromosome view every normal fertilised egg contains a double set of chromosomes, each carrying a complete set of the factor elements. Hence, if some of the one set become eliminated we can still imagine that a normal though undersized individual might develop. The converse relation where increased size goes with multiplication of chromosomes was discovered by Gregory,[24] in a Primula, and occurs also in *Œnothera gigas*, a mutant derived from *Œ. Lamarckiana*. Gregory found in his cultures giant individuals which behaved as though four instead of two sets of factors were present, and upon examination these individuals were found to contain twice the normal number of chromosomes. It is interesting in this connection to recall the results obtained by Nemec[25] as the result of subjecting the root tips of various plants to the narcotising action of chloral hydrate. Under this treatment cells undergoing division at the time were able to form the daughter nuclei, but the production of a new cell wall was inhibited. The cells thus became binucleate. If on recovery these cells were to fuse before proceeding to divide afresh a genuine tetraploid condition would result. So few cases of natural tetraploidy have so far been observed that we have as yet no clue to the cause which leads to this condition.

The conclusions to which we are led by the considerations which have here been put forward are, in the main, that we have no warrant in the evidence so far available for attributing special hereditary processes to the cytoplasm as distinct from the nucleus. On the other hand, there is a very large body of facts pointing to a direct connection between phenotypic appearance and chromosomal behaviour. In animals the evidence that the chromosomes constitute the distributional mechanism may be looked upon as almost tantamount to proof; in plants the observations on *Drosera*, *Primula*, *Œnothera*, *Sphærocarpus* are in harmony with this view. When we come, however, to the question of linkage and general applicability of the conception of 'crossing over' as adopted by Morgan and his school we are on less certain ground. In *Drosophila* itself, the case which the scheme was framed to fit, the entire absence of 'crossing over' in the male remains unaccounted for, while the evidence from certain plant types appears to be definitely at variance with one of its fundamental premises. If segregation at the recognised reduction division is definitely established for animal types, then we must conclude that the sorting-out process may follow a different course in the plant.

The question as to what is the precise nature of the differences for

[23] *Nerv. Phytologist*, vol. xix., 1920.

[24] *Proc. Roy. Soc.*, vol. lxxxvii. B, 1914.

[25] *Jahrb. f. wiss. Bot.*. xxxix., 1904, 'Das Problem der Begruchtungsvorgänge,' 1910.

which the Mendelian factors stand is constantly before the mind of the breeder, but we are only now on the threshold of investigation in this direction, and it is doubtful whether we can as yet give a certain answer in any single instance. Still less are we able to say what the actual elements or units which undergo segregation may be. In the case of such allelomorphic pairs as purple and red sap colour or white or cream plastid colour it may be that the difference is *wholly qualitative*, consisting merely in the formation or non-formation of some one chemical substance. But the majority of characteristics are not of this hard-and-fast type. Between some the distinction appears to be one of *range*—to be quantitative rather than, or as well as, qualitative in nature, and range must mean, presumably, either cumulative effect or a force or rate difference. It may well be, for example, that with some change in physiological equilibrium accompanying growth and development, factorial action may be enhanced or accelerated, or, on the other hand, retarded or even inhibited altogether, and a regional grading result in consequence. Range in a character is not confined to, though a common characteristic of, individuals of cross-bred origin. It may be a specific feature, both constant and definite in nature. For example, a change as development proceeds from a glabrous or nearly glabrous to a hairy condition is not of unusual occurrence in plants. In the Stock such a gradational assumption of hairiness is apparent no less in the homozygous form containing a certain weak allelomorph controlling surface character, when present with the factors for sap colour, than in those heterozygous for this or some other essential component. We see a similar transition in several members of the *Scrophulariaceæ*—*e.g.*, in various species of *Digitalis*, in *Antirrhinum majus*, *Antirrhinum Orontium*, *Anarrhinum pedatum*, *Pentstemon*, and *Nemesia*. In perennials an annual recurrence of this change of phase may be seen, as in various species of *Viola* and in *Spiræa Ulmaria*. It is somewhat curious that the transition may be in the same direction —from smoothness to hairiness—in forms in which the dominant-recessive relation of the two conditions is opposite in nature, as in *Matthiola* on the one hand and *Digitalis purpurea nudicaulis* on the other. Manifestation of the dominant characteristic gradually declines in the Foxglove, while it becomes more pronounced in, the Stock. In some, perhaps all, of these cases the allelomorphs may stand for certain states of physiological equilibrium, or such states may be an accompanying feature of factorial action. A change of phase may mean an altered balance, a difference of rhythm in interdependent physiological processes. In the case, for instance, of a certain sub-glabrous strain of Stock in which the presence of a single characteristically branched hair or hair-tuft over the water-gland terminating the midrib in a leaf otherwise glabrous is an hereditary character, it is hardly conceivable that there is a localisation in this region of a special hair-forming substance. It seems more probable that some physiological condition intimately connected with the condition of water-content at some critical period is a causal factor in hair production, and that this condition is set up over the whole leaf in the type, but in the particular strain in question is maintained only at the point which receives the

largest and most direct supply. In this same strain a leaf may now and again be found lacking this hydathode trichome in an otherwise continuous hair-forming series, an occurrence which may well result from a slight fluctuation in physiological equilibrium such as is inherent in all vital processes—a fluctuation which, when the genetic indicator is set so near to the zero point, may well send it off the scale altogether. If, as is not improbable in this and similar cases, we are concerned with a complex chain of physiological processes, investigation of the nature of the differences for which the allelomorphs stand may present a more difficult problem than where the production of a particular chemical compound appears to be involved. In such a physiological conception we have probably the explanation of the non-appearance of the recessive character in certain dominant cross-breds.

Up to this point we have treated of the organism from the aspect of its being a wholly self-controlled, independent system. As regards some characteristics, this may be regarded as substantially the case. That is to say, the soma reflects under all observed conditions the genetic constitution expressed in the Mendelian formula. Correspondence is precise between genotypic potentiality and phenotypic reality, and we have so far solved our problem that we can predict certainly and accurately the appearance of offspring, knowing the constitution of the parents. In such cases we may say that the efficiency of the genetic machine works out at 100 per cent., the influence of external environment at 0. Our equation somatic appearance = factorial constitution requires no correction for effect of conditions of temperature, humidity, illumination, and the like. But most somatic characters show some degree of variability. Phenotypic appearance is the outcome primarily of genotypic constitution, but upon this are superposed fluctuations, slight or more pronounced, arising as the result of reaction to environmental conditions. In the extreme case the genetic machinery may, so to speak, be put out of action; genotypic potentiality no longer becomes actual. We say that the character is not inherited. We meet with such an example in *Ranunculus aquatilis*. According to Mer,[26] the terrestrial form of this plant has no hairs on the [illegible] the leaf segments, but in the aquatic individual the segments end in needle-shaped hairs. That is to say, hairs of a definite form are produced in a definite region. Again, Massart [27] finds that in *Polygonum amphibium* the shoot produces characteristic multicellular hairs when exposed to the air, but if submerged it ceases to form them on the new growth. Every individual, however bred, behaves in the same manner, and must therefore have the same genetic constitution. In an atmospheric environment genotypic expression is achieved, in water it becomes physiologically impossible. A limitation to genotypic expression may in like manner be brought about by the *internal* environment, for the relation of the soma to the germ elements may be looked upon in this light. Thus in the case of a long-pollened and round-pollened Sweet Pea Bateson and Punnett [28] found that the

[26] *Bull. Soc. Bot. de France*, i. 27, 1880.
[27] *Bull. Jard. Bot. Bruxelles*, i. 2, 1902.
[28] *Report to the Evolution Committee*, Roy. Soc., ii., 1905.

F_1 pollen grains are all long, yet half of them carry the factor for roundness. If we take the chromosome view, and if it be presumed that the factor for roundness is not segregated until the reduction division, the cytoplasm of the pollen mother cells may be supposed to act as a foreign medium owing to a mixture of qualities having been impressed upon it through the presence of the two opposite allelomorphs before the moment of segregation. We should consequently infer that the round pollen shape is only produced when the round-factor-bearing chromosome is surrounded by the cytoplasm of an individual which does not contain the long factor. If, further, we regard the result in this case as indicative of the normal inter-relation of nucleus and cytoplasm in the hereditary process, we shall be led to the view that whatever the earlier condition of mutual equilibrium or interchange between these two essential cell constituents may be, an ultimate stage is reached in which the rôle of determining agent must be assigned to the nucleus. To pursue this theme farther, however, in the present state of our knowledge would serve no useful purpose.

Before bringing this Address to a conclusion I may be permitted to add one word of explanation and appeal. In my remarks I have deliberately left on one side all reference to the immense practical value of breeding experiments on Mendelian lines. To have done so adequately would have absorbed the whole time at my disposal. It is unnecessary to-day to point out the enormous social and economic gain following from the application of Mendelian methods of investigation and of the discoveries which have resulted therefrom during the last twenty years, whether we have in mind the advance in our knowledge of the inheritance of ordinary somatic characters and of certain pathological conditions in man, of immunity from disease in races of some of our most important food plants, or of egg-production in our domestic breeds of fowls.

My appeal is for more organised co-operation in the experimental study of Genetics. It is a not uncommon attitude to look upon the subject of Genetics as a science apart. But the complex nature of the problems confronting us requires that the attacking force should be a composite one, representing all arms. Only the outworks of the fortress can fall to the vanguard of breeders. Their part done, they wait ready to hand over to the [illegible] [illegible]h whom it lies to consolidate the position and render [illegible] secure. This accomplished, the way is cleared for the main assault. To push this home we urgently need reinforcements. It is to the physiologists and to the chemists that we look to crown the victory. By their co-operation alone can we hope to win inside the citadel and fathom the meaning of those activities which take shape daily before our eyes as we stand without and observe, but the secret of which is withheld from our gaze.

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